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'NERVES' AND THE PUBLIC HEALTH*

BY MAJOR GREENWOOD, F. R. S.

In the United States and Great Britain, the phrase 'nerves' is almost as over-worked a member of the man in the street's medical vocabulary as 'a touch of liver' used to be and it is not much more easily given a precise connotation. As a biometrician I have had to take an interest in 'nerves' and in order to try to make clear what the connotation seems to be, I find it more helpful to refer not to modern medical texts but to the works of writers of a remote past. More than two thousand years ago an investigator (1) whom most modern scientific writers depreciate, expressed the opinion that to assign 'soul' and 'body' to water-tight compartments was foolish and suggested that it was wiser to look upon 'soul' as the realisation, the entelechy of 'body.' Five hundred years later a practising physician (2), who was also something of a philosopher and did his best to destroy the mythology which asserted that a *wise* man—or, as a latter mythology had it, a *good* man—was not troubled by emotions, wrote a short treatise on the effects of a certain blind force within us, a something repugnant to conscious reason. These effects πάθη were, in his view, of much clinical importance. In part they were determined by the congenital make-up of the individual, in part by his environment, the circumstances of his life. They are not readily diagnosed by the victim; the impartial observation of another person is necessary and not easy to secure. Galen understood by πάθη what we mean by 'nerves' and was as alive as we ought to be to the fact that the diagnosis and treatment of 'nerves' are difficult matters. He, and the almost contemporary psychologist, Paul of Tarsus,

* The substance of this paper was contained in a Chadwick Lecture delivered on March 26, 1931 in London.

were aware that in a complex urban civilisation showing signs of dissolution, 'nerves' were very important causes of inefficiency.

We are living under a complex system of civilisation which shows ominous signs of dissolution and once again 'nerves' are of first rate importance. The biometrician does not seek the problems they offer—what biometrician *would* read medical and popular lucubrations on psychology if he had a decent excuse for not doing so?—they force themselves upon his notice.

For the last eleven years, I have been intimately concerned, as Chairman of the Statistical Committee of the Medical Research Council of Great Britain, with the investigations of the Industrial Health Research Board (formerly called the Industrial Fatigue Research Board) into the human factors which help or hinder industrial efficiency. These investigations have required us to study with attention the disabilities which hamper men and women in their everyday work. Often we have begun such a quest, hopefully enough; believing that what was amiss was something, perhaps complicated, but certainly within the sufficiently wide circle of ideas which texts of hygiene and physiology discuss. Nearly always we have found that there were other factors, factors appertaining to this odd world of 'nerves.'

THE DISTRIBUTION OF ACCIDENTS

My own interest in and connection with such studies took its origin in a purely mathematical, or arithmetical, problem. During the later years of the Great War, I served in a section of the Ministry of Munitions and, among various statistical returns which came my way, was one which set out the following information. A certain number of munition workers had served in a factory shop through a certain number of months; within that period such and such a number had sustained no accidents at all, so many had had one accident each, so many two accidents, etc. That frequency distribution suggested to me the following problem:—If we make hypotheses of the way in which accidents *might* happen, would the forms of frequency distribution of accidents as observed help us to decide which hypothesis were correct?

The following hypotheses suggested themselves:—

(1) That industrial accidents were *pure* accidents, in the ordinary use of language, viz. distributed by chance, like the numbers in a lottery. If on one day an individual had had an accident, that individual was no

more and no less likely to have another accident the next day than any other worker; all individuals had equal liability to sustain accidents.

(2) That *first* accidents conformed to schema (1) but that a person who had had an accident was thereby rendered *more* liable or *less* liable to have another accident.

(3) That all persons were *not* initially equally liable to have accidents; that some were accident-prone, others not.

Mr. Udny Yule and I then set to work to determine the frequency distributions which would correspond to each hypothesis. In our memoir of 1920 (3) we gave either an exact or an approximate solution of each case. I do not propose to reproduce here the algebra involved; it will be sufficient briefly to indicate the way in which a solution can be reached. Suppose the course of events in a population exposed to uniform conditions to be symbolised in such a schema as the following. In this it is assumed that the successive intervals of time are so short that not more than one accident can happen within each. We require to find the distribution of 0, 1, 2, etc. accidents after n intervals of time, where the intervals tend to become infinitesimally short and n accordingly tends to become indefinitely large.

Very little consideration is needed to convince one that if hypothesis (1) is valid, the distribution reduces to Poisson's limit to the binomial, viz. that when $p_0 = p_1 = p_2 = p_3 \ldots\ldots$ and the p's tend to zero, the distribution becomes:—

$$e^{\lambda_0}(1 + \lambda_0 + \frac{\lambda_0^2}{2!} + \frac{\lambda_0^3}{3!} + \ldots\ldots) \qquad (1)$$

where λ_0 is a constant.

It requires but little further reflection to convince one that if hypothesis (3) is true, i. e. if λ is not constant throughout the exposed to risk but ranges from $\lambda = a$ to $\lambda = b$ with a frequency $f(\lambda)$ which is an integrable function of λ, then the required distribution is obtained by multiplying (1) by $f(\lambda)d\lambda$ and integrating between the limits.

When $y_0 e^{c\lambda}\lambda^{r-1}$ is chosen as the frequency function of λ and its limits are taken from zero to infinity, the two parameters can be readily deduced from the mean and second moment coefficient of the accident distribution.

The expression of hypothesis (2) is less apparent and only in the particular case of $p_0 \neq p_1$, $p_1 = p_2 = p_3 \ldots$ or in the limit, $\lambda_0 \neq \lambda_1$, $\lambda_1 = \lambda_2 \ldots\ldots$ could we reach a solution in terms of the moments of

SCHEMA

TIME INTERVAL	ACCIDENTS					
	0	1	2	3	4	5
Δ_0	1					
Δ_1	q_0	p_0				
Δ_2	q_0^2	$p_0(q_0+q_1)$	p_0p_1			
Δ_3	q_0^3	$p_0(q_0^2+q_0q_1+q_1^2)$	$p_0p_1(q_0+q_1+q_2)$	$p_0p_1p_2$		
Δ_4	q_0^4	$p_0(q_0^3+q_0^2q_1+q_0q_1^2+q_1^3)$	$p_0p_1(q_0^2+q_1^2+q_2^2+q_0q_1+q_0q_2+q_1q_2)$	$p_0p_1p_2(q_0+q_1+q_2)$	$p_0p_1p_2p_3$	
Δ_5	q_0^5	$p_0(q_0^4+q_0^3q_1+q_0^2q_1^2+q_0q_1^3+q_1^4)$	$p_0p_1(q_0^3+q_1^3+q_2^3+q_0^2q_1+q_0q_1^2+q_0^2q_2+q_0q_2^2+q_1^2q_2+q_1q_2^2+q_0q_1q_2)$	$p_0p_1p_2(q_0^2+q_1^2+q_2^2+q_0q_1+q_0q_2+q_1q_2)$	$p_0p_1p_2p_3(q_0+q_1+q_2+q_3)$	$p_0p_1p_2p_3p_4$

the accident distribution, although we could and did obtain equations expressing each frequency of occurrence in terms of the λ's.

The next step was to test our hypotheses upon actual data. To that end, we had, of course, to select work people doing work (usually repetitive work at machines) under uniform conditions in an environment where trivial accidents—only trivial accidents not leading to absence from or incapacity for further work would serve our turn—were carefully recorded. Table 1 compares the actual distribution of trivial accidents amongst a group of women munition workers

TABLE 1

(1) 648 Women Working on 6-inch H. E. Shells for Five Weeks

NO. OF ACCIDENTS	OBSERVATIONS	SIMPLE POISSON SERIES	"BIASSED POISSON" (8)	"INFINITELY COMPOUND POISSON" (51) & (52)
0	447	406	452	442
1	132	189	117	140
2	43	45	56	45
3	21	7	18	14
4	3	1	4	5
5	2	0.1	1	2
(2) 414 Machinists Three Months' Study				
0	296	256	313	299
1	74	122	41	69
2	26	30	33	26
3	8	5	17	11
4	4	1	7	5
5	4	0.1	2	2
6	1		1	2
7	0		0.1	
8	1			
(3) 198 Machinists Six Months' Study				
0	69	53	71	66
1	54	70	49	61
2	43	46	41	38
3	15	20	23	19
4	13	7	10	9
5	1	2	3	4
6	2	0.5	1	1
7	1		0.2	0.6

with the distribution to be expected on each of the three hypotheses. The arithmetical expression of the first hypothesis may conveniently be termed a *Poisson,* that of the second a *Biassed Poisson,* that of the third an *Infinitely Compound Poisson.* Visibly the last accords best with the facts. Table 2 from Miss Newbold's report (4) illustrates this in a more striking way, for the test is more severe.

The workers came from different departments of a factory and, therefore, were exposed to different environmental conditions. The Infinitely Compound Poisson ignores these departmental differences, *its* parameters were deduced from the aggregate; the Poisson does not, the figures shown under that heading were obtained by summing the yield of Poissons applied separately to each department. Yet this Infinitely Compound Poisson, which takes account of individual variation but ignores environmental variation, represents the facts better than a method which ignores individual variations of liability but reckons with environmental variations.

There is an even simpler way of demonstrating that variations of personal liability are of great importance. If the cards are fairly dealt, there should be no correlation between the number of trumps I hold on Monday evening and the number of trumps I hold on Tuesday evening. If accidents are random happenings, my score in one month's work ought not to be correlated with my score in another month's work. Actually there is quite substantial correlation between the accident scores of different periods. This was first pointed out by Miss Hilda M. Woods and me (5), and the subsequent work of Miss Newbold and others has confirmed our findings, as Table 3 from Miss Newbold's report illustrates.

We may therefore regard it as proven that a factor of individual susceptibility is of fundamental importance in the determination of industrial accidents.

I need not spend time in demonstrating that this personal factor is not—as some critics naively suggested—extrinsic, due, for example, to variability not of the workers but of their machines. If we had a set of psychologically and physiologically identical motor drivers and supplied them with cars the brakes of which varied in efficiency, we should certainly expect an Infinitely Compound Poisson distribution of accidents and correlation between the accidents of one period and those of another. Miss Newbold tested that factor by correlating 'home'

TABLE 2

Accident Distributions Fitted to the Infinitely Compound Poisson Curve

Factory B (Whole Factory)

TABLE 2

Accident Distributions Fitted to the Infinitely Compound Poisson Curve

Factory B (Whole Factory)

NO. OF ACCIDENTS	MEN			WOMEN		
	Observed	Calculated by Infinitely Compound Poisson Curve	Sum of Poissons	Observed	Calculated by Infinitely Compound Poisson Curve	Sum of Poissons
0	239	250.5	202.0	111	113.5	80.5
1	98	92.9	130.4	66	63.4	84.3
2	57	46.1	65.6	42	36.8	53.5
3	33	24.7	29.5	17	21.7	26.4
4	9	13.8	12.4	10	12.8	12.0
5	2	7.9	4.8	9	7.6	5.5
6	2	4.6	1.6	4	4.5	2.6
7	1	2.7	.5 .7	4	2.7	1.3
8	0	1.6	.2	1	1.6 6.7	.6
9	4	.9	0	1 8	over 8 2.4	.2 1.0
10	0	.6 6.5	0	1	0	.2
11	1 7	.3	0	1	0	0
12	0	.2	0	0	0	0
13	0 over 12	.2	0	0	0	0
14	1	0	0	0	0	0
15	0	0	0	0	0	0
16	0	0	0	0	0	0
17	0	0	0	0	0	0
18	0	0	0	0	0	0
19	0	0	0	0	0	0
20	0	0	0	0	0	0
	447	447	447	267	267	267.1
	8 groups	$\chi^2 = 13.7$ $\chi^2 = 75.5$		8 groups	$\chi^2 = 3.1$ $\chi^2 = 40.0$	
		$P = .058$ $P = <.00001$			$P = .88$ $P = <.00001$	

TABLE 3

Correlation Coefficients Between the Accidents of the Same Workers in Two Periods

FACTORY	GROUPS	CORRELATION COEFFICIENT	NO. OF PEOPLE	LENGTH OF PERIODS (a)	(b)
D	I Females	.21 ± .15	19	2 years	5 months
D	II Females	.36 ± .09	42	2 years	5 months
E	I Males	.57 ± .02	445	1 year	1 year
E	II Males	.25 ± .04	288	1 year	1 year
E	III Males	.62 ± .03	226	1 year	1 year
E	IV Males	.20 ± .04	288	1 year	1 year
G	I Males	.36 ± .09	47	1 year	1 year
G	II Males	.57 ± .05	82	1 year	1 year
G	I Females	.53 ± .04	120	1 year	1 year
G	II Females	—.01 ± .10	50	1 year	1 year
H	Females	.05 ± .05	227	6 months	6 months

accidents with factory accidents and obtained again a positive correlation of order 0.2 to 0.3.

She was able to do this because in some of the factories workers came for treatment in the ambulance room not only for accidents incurred at the factory but also for those which happened at home. Of course even now there is an element of doubt. Perhaps what we are measuring is not so much liability to have accidents as liability to report accidents. Perhaps the people who, in our records, are shown as having many accidents do not really have any more accidents than their neighbours but are fonder than their neighbours of being pitied or of taking a few minutes away from work. That this is *part* of the explanation is suggested by the facts that there is significant correlation between the accident rate and the number of visits to the ambulance room for other causes, but no significant correlation between the accident rate and lost time from sickness. But I think we may surmise that this is only part of the truth and accept the view that those reporting more accidents did on the average incur more accidents than others. What then is this idiosyncracy which renders one prone to suffer and to report accidents, is it of the body or the soul? *

The investigations which I have been describing led to the conclusion that there was but negligible correlation between output and accident

* By *soul* I mean here, and throughout, the non-corporeal activities of man; within that territory one is concerned with the conative and affective rather than the cognitive functions.

rate and, as we have seen, but negligible correlation between the latter and days of sickness. *Prima facie* then it did not look as if the body, statistically speaking, were much affected. The work of Eric Farmer and E. G. Chambers (6) brings out the result that an accident rate is correlated with performance in certain tests which explore rather the affective than the rational sides of the soul. For instance, the psycho-galvanic reflex seems an empirical test of the emotionally unstable; those marked down by it seem to have a higher incidence of trivial accidents.

TABLE 4

Average Accident Rate Expressed as a Percentage of the Mean of the Group of those with Different Degrees of Tremor (Group F)

	SUBJECTS	ACCIDENT RATE
No tremor	102	98
Slight tremor	39	81
Tremor	53	96
Marked tremor	16	213
	WITH MARKED TREMOR	WITHOUT MARKED TREMOR
Weighted averages	213	94
Difference		+ 119 †

† As there are only 16 with marked tremors the significance of the difference is worked out by means of the tables given in *Biometrika*, XI, pp. 416-417. The expectation that we should get the above difference is 3 in 1,000, hence the difference can be taken as significant.

TABLE 5

Average Accident Rate Expressed as a Percentage of the Mean of the Group of those Above and Below the Mean in the Number of Changes in Conductivity Towards Lowered Resistance in the Psycho-galvanic Reflex Test

GROUP	ABOVE	BELOW
A	36	159
B	90	116
Weighted averages	70	137
Difference	+ 67	
P. E. of Difference	26.8	

Using a series of psychological tests and giving a weight to each proportional to its correlation with the accident rate, Farmer and Chambers reached the results shown in Table 6.

TABLE 6

Average Accident Rate Expressed as a Percentage of the Mean of those Better and Worse than the Average in the Combined Weighted Scores (Weighted by Means of the Coefficients in the Regression Equation)

GROUP	BETTER IN TESTS	WORSE IN TESTS
C	85	120
D	64	145
E	91	111
F	72	128
Weighted Averages	77	125
Difference	+48	
Probable Error of Difference	11.6	

There can be no doubt of the importance of πάθη or "nerves" in the aetiology of trivial accidents. This point having been reached, I shall be told that it was not worth while spending so much time to reach it. The results of psychological, particularly medical or industrial psychological investigation, pass through three stages of opposition, like a bill in parliament. At the first reading, they are said to be absurdly untrue; at the second reading, they are said to be abnormal and improper; at the third reading, they are said to be trivialities well known to everybody since the Flood.

We have reached the third stage here. What is all this fuss about; everybody knows that some folks' fingers are all thumbs, why prove it by algebra and laboratory tests? The erudite critics will quote *Hudibras* and others will recall a more modern classic:—

> A truth of such undoubted weight,
> He urged, and so extreme in date,
> It were superfluous to state.

The answer is that this minor peculiarity is of diagnostic importance. Farmer and Chambers, having more material than Miss Newbold, were able to show that the incidence of these trivial accidents was correlated with the incidence of more serious accidents which did involve real loss of working time and no doubt included some which were serious in every sense of the word. Accident-prone people *are* dangerous people. We now know that their proneness is a function of "nerves," we *must* try to recognise them before they have accidents, for the sake of themselves and others. I give one last illustration (Table 7). It is not so homogeneous as the others, but it is impressive.

TABLE 7

Comparison of those who had Failed in the Passing-out Examination of the R. A. F., and those who were Top in the Next Entry

	REACTION	DOTTING	PURSUIT METER	INTELLIGENCE	NUMBER SETTING
Failures	98.8	99.5	98.1	87.5	95.1
Top in next entry *	100.3	100.0	102.4	113.2	105.3

	TOTAL ACCIDENT RATE	SICKNESS RATE	MAJOR ACCIDENT RATE
Failures	110.9	121	141
Top in next entry	87.5	77	51

* Practically all of these have since passed their final examination.

THE PERSONALITY FACTOR IN OCCUPATIONAL DISABILITIES

I have spent so much time upon this problem of accidents because, although we are only at the beginning of things so far as the devising of practically efficient tests of vocational aptitude is concerned, the *general* problem of the aetiology of trivial accidents has been solved. The next example, that of the Industrial Health Research Board's work on occupational disabilities, such as telegraphists' cramp and miners' nystagmus, I must deal with more concisely because not only is the work in progress but, unless one can give a great deal of space to it, false impressions will be conveyed.

One serious obstacle to enlightenment is a tendency, unhappily by no means confined to non-medical circles, to assume that if one attributes an important share of the aetiology of some disabling and, in the literal sense of the word, dreadful illness, to 'nerves,' one is accusing the sufferer of shamming. I shall therefore say no more than this. Research work of the kind which I shall discuss in detail a little later, is making it increasingly clear that, so long as we regard each of these disabilities as *sui generis*, and explain its aetiology wholly by reference to the particular environment of the occupation or to the particular functioning of mechanisms of physiological order, we shall miss something of first rate importance. In each of the important occupational disabilities which have attracted attention one finds bizarre groups of signs and symptoms and there has been a tendency to concentrate upon the mechanism of these. For instance in telegraphists' cramp attention was concentrated upon the physiological mechanism, in miners' nystag-

mus attention was concentrated upon the conditions of illumination. That as a result valuable discoveries have been made and important improvements of working conditions effected, I, at least, am satisfied. But it is not less clear that insufficient attention has been given to aetiological factors common to *all* the occupations. One may vary the stage properties, the costumes and the rôles of a company of actors, but their fundamental idiosyncracies can be discerned through all.

THE MYTHOLOGY OF STATUS LYMPHATICUS

A reason for this neglect is the tendency which most of us would admit to regard the intangible as outside the pale of medical science. We do not object to generalisations, as such, but do expect evidence of the truth of such generalisations to be shown us 'on a plate.' Sometimes this has led the medical profession into sad confusion as in the rather discreditable instance of what is called *status lymphaticus*, or *status thymicus*.

Down at any rate to the middle of the eighteenth century, the Greek doctrine of temperaments—what in our generation is called constitutional pathology—was part of medical education. Galen's *de Temperamentis* was still valued when his anatomy and physiology had long been superseded. But by the end of the nineteenth century all this was forgotten by most medical men, although the great Virchow did not quite neglect it. Some 41 years ago a Viennese physician, Paltauf, propounded the question whether persons apparently healthy who died suddenly without any obvious reason, e. g. after taking a bath, might not present some common characteristic other than the seemingly inexplicable death. He made the purely tentative suggestion that in such persons the thymus was abnormally large and that this enlargement, together with indications of over development of the lymphatic system and perhaps some narrowing of the aorta, might characterise a peculiarly labile type of organisation, an *Entartung*, a deviation from the normal habit of body, such that stimuli ignored or favourably interpreted by the normally constituted microcosm were unfavourably interpreted, even to the point of death. There was nothing unreasonable in the idea, but unfortunately the current desire for an objective explanation led some people to hold that a diagnosis could be based upon a mere weighing of the thymus by persons wholly unacquainted with the weight of the thymus in those who had *not* died under inexplicable circumstances. It led other people equally unfamiliar with the histology of the thymus to

fantasy other pathognomonic criteria and still more people having a mere nodding acquaintance with experimental physiology to contrive wonderful hypotheses of endocrinology. Mere measurements of weight (7) and exact histological researches (8) have quite conclusively shown that the *status lymphaticus* of the medical profession belongs not to science but to mythology. They have *not*, however, shown that the problem originally proposed by Paltauf is not a real problem. On the contrary the suggestion clearly is that here we have a problem which can only be solved by an approach on the lines of Galen, viz. through a study not of one but of all aspects of human temperament.

This is, of course, an extreme and, practically, not very important case, but it illustrates why the Industrial Health Research Board have thought it wise to study ill-health from a point of view not very congenial to those bred in the traditions of the late nineteenth and early twentieth centuries.

'NERVES' IN INDUSTRY

The first fruits of this method of study are to be found in Dr. M. Culpin's paper read before the Section of Psychiatry of the Royal Society of Medicine in 1927, (9) Dr. May Smith's monograph (10) on *The Nervous Temperament* published last year, and the report on *The Nervous Temperament* made by the investigators named to the Industrial Health Research Board and published (11) in 1931.

The direct charge upon industry of illness falling within the scope of 'nerves' can be approximately measured, but only approximately, because the system of recording loss of time through sickness varies from firm to firm. Dr. Culpin prepared the following data from the records of a firm employing about 960 persons of whom 85 per cent were females. The annual average loss of time through sickness is given as 7.2 days. It will be seen that the selected causes account for

TABLE 8

	1923	1924	1925
Anaemia	257	386.5	418
"Gastric"	476	624	890
Dysmenorrhoea	197.5	174	177.5
Neurasthenia "Nerves," General Debility	1705.5	1206	1070.5
Totals	2636	2390.5	2556.0

2.7, 2.5 and 2.7 days per head. "After 1923 the medical staff paid attention to the prevalence of the 'neurasthenia' sick-leave, with the result that it diminished as is shown; but the increase under the heads of 'anaemia' and 'gastric' left the total practically unaltered."

In another business, that of a firm with excellent welfare arrangements and a special medical staff, female employees lost 14.41 days per annum in the age group 18-22 and 16.48 days in the age group 22 and upwards, during the year 1924. Of this loss 14.9 per cent was assigned to dysmenorrhoea.

Another, unpublished, example is shown in Table 9.

TABLE 9

Long Sick Leave (i. e. 30 days + at one spell)

NO. OF CASES OF SICK LEAVE	MEN	WOMEN
	522	941
	17 = 3.3% of total number	79 = 8.4% of total number
	Days	*Days*
Distribution	1011 = Total of sick leave	5774 = Total of sick leave
Nervous Breakdown	260 = 25.7%	1841 = 31.9%
Influenza + Debility	126 = 12.4	513 = 8.9
Operations	91 = 9.0	853 = 14.8
Bronchitis	93 = 9.2	137 = 2.4
Influenza	0	251 = 4.3
Anaemia and General Debility	0	293 = 5.1
	56.3%	67.4%

The other disabilities either refer to one person or account individually for less than 1 per cent.

Such data as these are far from satisfying the desires of a professional statistician; an improvement of current systems of recording and tabulating sickness data in industries is long overdue; but they are sufficient to prove that even the direct charge upon industry is a heavy one. Once more, as in the problem of accident causation, of occupational disabilities in the technical sense, e. g. telegraphists' cramp, miners' nystagmus, we are compelled to ask ourselves what *sort* of people have these troubles? Can we characterise them?

THE DIAGNOSIS OF PERSONALITY

The Greek physicians based their diagnosis and prognosis of troubles of both body and soul upon an estimate of the patient's temperament, a

knowledge of his environment and a synthesis of the signs and symptoms of ill-health. In collecting data they had to depend upon their unaided senses. In the appraisement of physiological characteristics and of the tangible or visible consequences of disease our physicians have long had at command technical instruments of precision which enable them to collect data inaccessible to the Greeks. Even in the assessment of some of—shall we call them—attributes of the soul, modern techniques are of value; everybody has heard of an Intelligence Quotient and most people have heard of the Word Association test. Both these techniques have provided data from which, by the application of more or less sophisticated *statistical* reasoning, more or less reasonable inferences have been drawn. I do not wish to sneer at these evidences of the March of Mind, and have, I hope, a proper share of statistical *esprit de corps*. Still it would be a little optimistic to suppose that even the best of statistical mathematicians analysing the data compiled by the most conscientious of Ph. D. candidates could add up human beings *quite* satisfactorily. The diagnosis of a physician with an experiencing nature, who understands but is not the dupe of technical methods, is a datum for which no substitute which is just as good can be found. In the work of the Health Research Board, the assessment of a physician has been regarded as a datum of primary value. But, of course, the value of such a datum must be itself tested. While it is foolish to believe that, given a sufficiently elaborate technique, brains can be dispensed with, it is equally foolish to suppose that we can hand over our problems to supermen and accept their conclusions as inspired truths. Both these mistakes are being made every day. One leads to elaborate trifling, the other to dogma.

If our physician assesses a group of examinees, asserts that some of them are more likely to be troubled with 'nerves' than others, we are not merely to take his word for this, any more than we are to believe the statistician who having demonstrated some measurable difference between two groups to be, in *his* sense, significant, implies that 'significant' and 'important' are interchangeable terms and that if we do not see that, so much the worse for us.

The most obvious way of testing the physician is to determine whether *his* assessment bears any relation to an assessment of the same group made by different people with different objects in view, independently of him. The man in the street would, I think, reason in this way: "The doctor has examined 100 people working in a factory and he

says that such and such a percentage are nervy. Very well, let the employer add these people up in *his* way and let us see whether the people to whom the doctor gives bad marks are the people to whom the employer gives bad marks." That is a sporting enough offer and should be accepted. In this research the method of investigation has been as follows: Examinees have been added up in two ways. Each has been interviewed by a physician, Dr. Culpin, who has assessed them on his system. Each has also been interviewed by an experimental psychologist, Dr. May Smith, and provided her with a record on the McDougall-Schuster dotting machine. The procedure is best described by the investigators themselves. ((11) pp. 7-9)

Details of Technique and Method. Information with regard to these various attitudes to life was sought from each subject in a personal interview lasting about 20 minutes. A rigid questionnaire was not used, for the variety of symptoms and of their mode of expression is infinite. As the work progressed, however, a general scheme was evolved by which the subject was led through various hypothetical situations described above that would stimulate the symptoms most commonly encountered. If a subject talked easily he was allowed to follow his own path, questions being interpolated either when he stopped or when some point required elucidation. It sometimes happened that a subject denying the existence of difficulty with regard to any of the proposed situations and appearing to lack any symptoms, when asked as a matter of routine at the end of the interview if there was any question he would like to put, would ask for explanations of symptoms that happened not to be expressed in the precise form implied in the questions. There was no intent to deceive; the particular symptom did not seem to the sufferer to be sought after.

Subjects sometimes described themselves as nervous because of a feeling of discomfort at examinations or of slight uneasiness when undertaking something new. Such characteristics, in so far as they did not deter the subject from action or interfere with his happiness when the occasion was over, were not called symptoms: they are the common human reaction to the unknown and can be modified by experience. A characteristic of the nervous symptom is its relative fixity and irrationality, usually recognised by the subject as such.

In addition to being interviewed, each subject was tested with the McDougall-Schuster dotting machine, which experience during the cramp investigation has shown to be, for practical purposes, the best objective check upon the results obtained by the interview, as well as, in some cases, a means in itself of differentiating types.

This test was devised by Professor William McDougall * for the purpose of

* Wm. McDougall. "On a New Method for the Study of Concurrent Mental Operations and of Mental Fatigue." *Brit. J. Psychol.*, Vol. I., Pt. 4.

Wm. McDougall and May Smith. "Effects of Alcohol and Other Drugs during Fatigued and Unfatigued Conditions." *Medical Research Council. Special Report* No. 56.

"A Study of Telegraphists' Cramp." *I. H. R. B. Report* No. 43.

testing sustained voluntary attention and muscular control. The form used was the Disc-dotting machine, a modification of the original, designed by Dr. Schuster.† An irregular row of small circles 2 mm. in diameter, is printed spirally in red on a paper disc 326 mm. in diameter. The spiral makes altogether two complete turns. The interval between successive circles, in the direction of the motion of the spiral is 5 mm.; the lateral deviation is never more than 7 mm. The circles are thus clustered irregularly about the "true" line of the spiral. The paper disc is mounted on a plate fixed horizontally, which rotates at a constant rate. A cover is supported above the plate in which a slot 1 in. wide is cut in such a position that, as the disc rotates, every point in the spiral row of circles presents itself to view through the slot. The subject rests his wrist on the cover and aims at the circles as they appear through the slot at one edge and are carried out of sight at the other. He starts at the inner end of the spiral and makes his way outwards over it as its whole length presents itself point by point through the slot. At the inner end of the spiral an angular movement of 45° brings 6 circles out from under the edge of the slot; at the outer end of the spiral 24 circles. Thus, the subject has to work at a gradually increasing speed. If the subject touches the circle it counts as correct: he is not expected to hit the centre.

Sometimes an interview was requested as a special favour for the purpose of seeking help, either by a subject himself, or by someone in authority who found him difficult. These cases, which for scientific purposes often proved very valuable, were kept separate and not included in the percentage classification for the firm.

For statistical purposes subjects were graded from 0 to 5, according to the absence or the severity of symptoms. "0" meant that within the limitations of a short interview conducted for research purposes no particular symptoms were discovered. This group comprises a very wide range of people. At one extremity is the emotionally well-poised, sensitive and highly intelligent man, at the other is the rather dull person. It has not been considered desirable to call this group "normal" as that implies more knowledge than we have. With increasing knowledge it may be possible to get some conception of the "mode," i. e., the commonest type in a random sample of the population.

"1" was used for those who either had only one symptom and that of such a nature as to affect their lives but little, or had several to a very slight degree affecting them occasionally. With adolescents it was sometimes difficult to judge whether a symptom would develop or for practical purposes disappear. If a subject had definite nervous symptoms he was marked 2, 3, 4, or 5, according to their severity, those marked 5 being serious cases, i. e. needing treatment. Rational anxiety due to current stress would not be called a symptom.

Obviously, such a classification can only have approximate value. A few subjects (less than 0.5 per cent of the whole) appeared to have no symptoms, and yet gave the impression of either wittingly (when there was an over-emphasis on the absence of symptoms) or unwittingly keeping something back. Something in their appearance or behaviour suggested emotional difficulties, for which adequate evidence was not forthcoming; they were marked as doubtful and not classified.

† We are indebted to Dr. Schuster not only for designing the machine but for describing it.

In some cases the interviewer was aware of an indefinable difficulty in getting into touch with a subject whose attitude to life, whilst not affording any definite evidence of a disordered mental balance, seemed peculiar. Since in the number of cases examined it is probable that a few will at some time or other become victims of a psychosis, it is possible that these difficult subjects include some of those few. In two cases the existence of an early psychosis was suspected, one patient possibly being a paraphrenic and the other a case of dementia praecox.

Before we consider the yield of these analyses in detail, I propose to take up the sporting offer of my man in the street. Among the persons examined by Dr. Culpin and Dr. Smith were 158 whom employers or superiors had classified. The industrial classification was to the effect (I shall only deal with broad results, the details can be found in the reports cited) that 91 of the 158 were either good or very good officers; for brevity, we may say that the 91 were *efficient*. Among the same 158, 38 persons received bad marks from both the clinical and the experimental psychologists. Now let us apply the man in the street's test in this way. If the psychologists' criteria are of any practical value then, by excluding the 38 marked down by them, we ought to find that the percentage of efficient officers in the remaining 120 is greater than in the original set of 158. In fact we find that the percentage of efficients is increased from 57.6 per cent to 63.3 per cent. But might not this be due to 'chance'? If we draw at random from a pack of cards, say 10 cards, the percentage of spades in the ten might be more than 25 without the drawer having any special power of discerning which of the cards were spades. To answer that question is a purely statistical operation. I find that the chance is less than one in a hundred that if 38 officers had been excluded capriciously the percentage of efficient in the remaining 120 would have been increased as much as (or more than) it was actually increased by exclusion based on the psychological diagnoses.

It is easier for me and, I think, easier for my man in the street to believe that the selection was *not* a random one than that the one in a hundred chance came off.

Here is another example. Fifty-two male and 36 female entrants to the telegraph service who had been examined by Dr. Culpin and Dr. Smith (12) were reported upon by their superiors at the end of a year's service; 19 of the former, or 36.5 per cent and 22 of the latter or 39.3 per cent, were reported to be very good or good officers. Elimination of the officers who received the worst marks on Dr. Culpin's and Dr. Smith's scales reduced the totals to 36 and 47 respectively and increased

the percentages of very goods and goods to 44.8 and 40.4. If we club together the two groups it is found that the chance that such an improvement might have been effected by a mere random exclusion is approximately the same as in the last instance: it is about 1 in 100. Taking into consideration the other instance independent of this, and yet further independent examples which might be quoted, we reach the result that the odds against a "chance" explanation are so large that it may be excluded and we can be confident that the psychologists' criteria and those of practical men are *not* independent but, to speak technically, correlated. I wish, however, to emphasize the fact that we are at the beginning of a long journey. Take for instance the last quoted example. A mere partisan might fasten on the experience of the men and say that the psychological method had been enormously successful. Its adoption for vocational selection would have increased efficiency by more than a fifth. By using these methods, he might say, your business efficiency would go up by twenty per cent. Another partisan, looking at the women, whose percentage efficiency changed only from 39.3 per cent to 40.4 per cent, might retort that the whole business was mere quackery. Both would be unscientific. *We are at the beginning of a long journey;* we have got no further than to make it highly probable that there is a relation between psychological make up *as defined by these methods of examination,* and occupational adaptation. We have, I think, the right to expect the man in the street to hold that there is 'something in' this method and when it is found, as it is found, that many more than 25 per cent of people actually doing the work of the world in factories, government departments and offices have 'nervous' symptoms, it is urgent that we should ascertain how much there is 'in' it.

Various questions now arise; one is a practical one. I have described two methods of psychological assessment; one, that of the dotting machine, might—but *not* safely; sharp tools cut inexperienced fingers—still *might* be used by a layman. The other can only be employed by a physician. Are the results of these tests so highly correlated that we could afford to rely on one only? The answer is quite unequivocal; the results of the two tests are correlated, but the measure of correlation, although substantial, is very far short of unity; it is not so close as the correlation between the stature of an adult and that of his adult son. The two methods are not testing precisely identical qualities, we cannot dispense with either. But, there is sufficient correlation between them to enable us to assess the assessor; a clinical classi-

fication which is not correlated with the dotting machine classification is suspect.

It would be tedious to enumerate the measurable associations between the criteria of 'nerves' which these investigations have defined and signs of mal-adjustment to the economic environment. They are set out in detail in the Board's recent publication (11); all I wish to say is that, just as in the instances already discussed, we have a mass of evidence no single item of which is, taken alone, conclusive, while the cumulative force of all the several items is great. Now we come to an important point. It is *not* true that because the proportion of employees in a business who exhibit signs and symptoms of 'nerves' is large that the loss of time by sickness, even by sickness which is certainly referable to 'nerves' is necessarily large. Perhaps it may be urged that this is a *reductio ad absurdum* of the whole business. If it is, I can prove by the same 'logic' that bodily hygiene is all nonsense. I could give proof that the sickness lost time of an establishment the hygienic and welfare arrangements of which would make a well trained medical officer of health jump for joy, may be, and sometimes is, *far* higher than that of an establishment which he would like to proceed against under one of the hundreds of enactments for the harrying of bad employers, which our legislature has provided.

What *is* proved is that the 'nerves' of employers are rather more important than the 'nerves' of employees and that a limitation of our field of inquiry to the latter would make it impossible to solve the whole problem. Social hygiene, more than any other branch of medicine, is the happy hunting ground of one-ideaed people.

'NERVES' AND THE MODERN MILIEU

"Sir Arthur St. Clare, as I have already said, was a man who read his Bible. That was what was the matter with *him*. When will people understand that it is useless for a man to read his Bible unless he also reads everybody else's Bible? A printer reads a Bible for misprints. A Mormon reads his Bible, and finds polygamy; a Christian Scientist reads his, and finds we have no arms and legs." Mr. Chesterton's Father Brown had insight. In this age of the world, 'nerves' are more important than in the age of William the Conqueror not because the pace of life is faster than in 1066 but for the precisely opposite reason. It is so much slower. Life in old England *may* have been merrier but it was certainly shorter than it is now, the struggle for bare existence

keener. Hunger, according to a mediaeval saying, was the Englishman's plague. There is nearly always some truth in popular harshness and folly; the sneer of the tough-minded at the troubles of the tender-minded, whether called vapours, hysteria or 'nerves', to the effect that only idle people have time to indulge their 'nerves' has a percentage of truth. "Depend upon it, Sir, when a man knows he is to be hanged in a fortnight it concentrates his mind wonderfully" is Johnson's presentation of the same common sense philosophy. When a man is struggling for his life in deep water, he is either too busy to entertain the haunting thoughts which led one of the subjects of the Board's investigation to dissect her actions and then "dissect the dissection . . . . till I nearly go mad," *or* he just drowns. Where the common-sense philosophy lets us down is in not enabling us to forecast *which* event will happen. But, statistically speaking, it does not matter; the water test *would* much reduce the incidence of 'nerves' upon a living population and it was applied in the past.

Now we do not apply this test. Hardly any people in this country, speaking statistically, are in imminent peril of actual starvation, but a great many, *millions* of people in this country have only too much time to dissect the dissections of their feelings. Not only those on the dole, but those at work for which they are ill-fitted but which is the only work available. Perhaps 10 per cent of the inhabitants of Mersey side are doing little more than existing, for 16 per cent of the families there are below the Poverty Line, according to a recent social survey.

Millions of people now have leisure for reflexion. They may not be acutely hungry, they are not in imminent peril of death, but they are insecure. Every Englishman over 40 remembers the war-time atmosphere of living in a draughty railway waiting room, waiting for the next train, not knowing when the next train would come. One had plenty of time to stare at and be irritated by the ugly advertisements, to curse the noise of other people's trains, to poke the fire without raising a glow from it, but not time to settle down to read comfortably. We fancied peace would dissipate that atmosphere; it has generalised it. A very large minority of the whole nation are sitting in that waiting room; the time may come when it is a majority. These people are turned in upon themselves; like the proverbial yokel, they sit and think or sometimes they just sit. They have leisure, but they have not been taught to use that leisure. Uneducated townsfolk *cannot* amuse themselves, and the amusements provided for them, especially those provided by

cinema corporations, are remarkable. More than a quarter of a century ago, a physician told us students that there was no abnormality of the soul which was not the growth of a germ in the soul of everyone of us. The same idea has been expressed in the dialects of moralists of all schools. 'Passionate' scenes at the 'pictures' are rigorously censored, the details of divorce court evidence must not be printed. But the world and its adolescent children may see realistic pictures of men burning to death in aeroplanes or being shot by files of soldiers and may read of women being stripped and painted black. The theory is, I suppose, that it would be inconvenient if young people emerging from picture houses were to assault criminally the passers-by, which *might* be, it is supposed, the consequence of exciting their normal sexual passions, while for them to reflect in their abundant leisure upon torture and bizarre emotions, is of no public importance. The seed of 'nerves' is planted in most of us; we water it with an unhealthy leisure and manure it with bizarre excitants and, when the crop is raised, denounce pneumatic drills, motor-hooters, communist Sunday Schools, psycho-analysts, everything and everybody but the right reason.

THE PROGRAMME OF INDUSTRIAL PSYCHOLOGY

This, however, is an emotional digression. We cannot mend society, but as medical investigators we can try to diagnose 'nerves' and mitigate some of their evil consequences. The researches which I have described have brought us to this point. By a combination of clinical and experimental methods of psychology means of stochastic diagnosis have been obtained. I use this hard word, which is, to me, essentially equivalent to statistical, to free myself from the ordinary associations of so common a word as statistical. The point is that, partly owing to our present ignorance of adaptations, prognosis following upon diagnosis cannot yet, in this branch of medicine, have the precision of at least *some* forms of physical diagnosis. We find that a is more often associated with A than with B and that b is more often associated with B than with A, but we do not find that the associations of a with B and of b with A are impossible. We have some knowledge of the attendant circumstances which favour or impede the cross-linking, but not nearly enough. Whether we can broaden our knowledge depends upon whether others can be trained to apply and improve these methods. So far as concerns the provision of research workers for the service of such organisations as the Industrial Health Research Board, the future

is not unfavourable. There already exist means of training young investigators, and, in the very near future, these means will be strengthened. In my opinion—and I think this opinion is shared by my psychological and medical colleagues—this training must consist of a thorough grounding in the principles of physiology and general psychology, a course in statistical logic and methods and personal experience of industrial conditions under the supervision of a clinical psychologist. The existing academic diploma in psychology of the University of London, when industrial psychology is taken as a special subject, involves a quite adequate course of study. But the provision of trained investigators, although an important, is far from being the most important need of the time. We must excite interest in and raise the standard of knowledge of medical officers in the public health services and in the employment of great businesses and firms.

A beginning has been made at the London School of Hygiene and Tropical Medicine, where the teaching of psychology forms an integral part of the training for the Academic diploma in Public Health. The aim is not to produce investigators, but to convince those who will have to deal with the results of ill-health that these troubles of the soul are both interesting and practically important and to give them some idea of how their aetiology may be explored.

This is only a beginning. In my opinion, physicians, psychologists and statisticians working together can help employers and employed materially, but only on the condition that they recognise and even proclaim their ignorance.

For many years, throughout the relatively static and prosperous industrial age which for the United States and Great Britain might be taken to be the period between the War of Secession and the European War, 'nerves' were ignored, mistaken and derided by the orthodox medical profession, whose philosophy tended to be naively materialistic. The dissemination of the discoveries and hypotheses of Freud towards the end of the epoch, excited a vehement controversy, but the adherents of the 'new' medical psychology (some of which was only 'new' in a geological sense) were a minority and still are a minority. They have therefore some of the virtues and vices of a minority and are prone to exaggeration and partial statement. We cannot do any good if we replace one set of witch doctors by another set.

REFERENCES

(1) ARISTOTLE. *de Anima.* Lib. ii. Cap. 1.
(2) GALENOS, CL. *Opera Omnia,* Kühn's Edition. Vol. 5, pp. 1 *et seq.*
(3) GREENWOOD, M., and YULE, G. U. *Journ. Roy. Stat. Soc.,* LXXXIII. 1920. 255.
(4) NEWBOLD, E. M. *Industrial Fatigue Research Board,* Report No. 34. *Journ. Roy. Stat. Soc.,* XC, 1927. 487.
(5) GREENWOOD, M., and WOODS, H. M. *Industrial Fatigue Research Board,* Report No. 4, 1919.
(6) FARMER, E., and CHAMBERS, E. G. *Industrial Fatigue Research Board,* Report No. 38, 1926.
(7) GREENWOOD, M., and WOODS, H. M. *Journ. Hygiene,* XXXVI. 1927, 305. YOUNG, M., and TURNBULL, H. M. *Journ. Path. & Bact.,* XXXIV. 1931, 213.
(8) HAMMAR, J. A. *Die Menschen-Thymus,* 2 vols. Leipzig. 1926-9.
(9) CULPIN, M. *Proc. Roy. Soc. Med.,* XXI, 1928, 13.
(10) SMITH, M. *British Journ. Med. Psychology,* X. 1930. 99.
(11) CULPIN, M., and SMITH, M. *Industrial Health Research Board,* Report No. 61. 1931.
(12) CULPIN, M., SMITH, M., and FARMER, E. *Industrial Fatigue Research Board,* Report No. 43. 1927.

APICAL DYSTROPHY

AN INHERITED DEFECT OF HANDS AND FEET

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A very conspicuous inherited deformity of the fingers and toes has occurred fifteen times in three generations of a local family.* Since this defect is of a comparatively rare type and has been only imperfectly described and illustrated, but can now be sharply differentiated from the classical forms of brachyphalangy, with which it is commonly confused, we are contributing the following report, review of literature, and comparative analysis.

DESCRIPTION OF THE DEFECT

This unusual form of shortened fingers (Fig. 1) is manifested in affected members by the absence of the terminal portions of the fingers (digits II to V) and complete or nearly complete absence of the finger nails; the thumbs, however, are (with one exception) never shortened, though they are often abnormally broad and flat, and they always bear nails, which are sometimes more or less split and doubled. The feet, though similarly involved, are outwardly much less abnormal (Fig. 2); certain of the four outer toes—never the great toe—are often noticeably reduced in length, and their nails vary roughly in proportion to the length of the toe, from full size to a rudiment, or may be completely absent, as is typical in the fingers.

The fingers are usually so short that they project little beyond the tip of the thumb. The reduced digits are not dwarfed or stunted as a whole; they may best be described as resembling the stout stumps remaining after amputation of the terminal third, half, or two-thirds. The family often refers to them appropriately as banana-like. The

* Attention was drawn to the first case by the family physician, Dr. McCullough, who has coöperated in obtaining the complete family history and the illustrations, and is describing the case histories in another place.

stump ends may be blunt, rounded, or even decidedly pointed. They may present a quite even front, but more commonly the middle or the index finger is longer than the ring and little fingers; that is, the digits of the radial side of the hand tend to be more perfect than the ulnar ones. There is very marked, but by no means perfect, bilateral symmetry in the two hands of the same individual. The finger nail, when present at all, shows merely as a minute scaley flake on a rough nail bed sunk deep in the bottom of a pit on the dorsum of the finger tip. Such nail rudiments occur only on fingers possessing two whole phalanges or more. The thumb nail is always present and large.

In all cases the hand and the proximal third of each finger is intact and normal. Usually a larger or smaller portion, if not the whole, of the middle phalanx is present, and sometimes the basal part of the third phalanx as well. Practically the extreme range of variation in hypophalangy encountered in this group of cases was found in a single family in the hands of three brothers and their father (Figs. 1 to 4). In fact one hand may show tri-, bi-, and nearly monophalangic digits. In the palmar views are seen the joint creases—only one on most fingers, two on a few. The lines of the hand seem normal.

The feet are less seriously affected by the causes that alter the upper extremities so markedly, so that little vertical correspondence is found. The fourth toe commonly, and the third and fifth occasionally, are visibly shortened and may carry more or less shell-like scales for nails or be quite nailless. The reductions in the toes are too extreme to be included within the wide limits of normal variability. X-ray photography is highly essential to reveal the actual reductions that occur in the toes.

ABNORMALITIES REVEALED BY X-RAYS

Most of the individuals were examined only by inspection and palpation, but the four mentioned above were selected for further study by radiographs because the range in their ages (7, 12, 17 and 47 years) permitted a determination of most stages in the ossification of the phalangeal elements. Radiographs of the malformed members (Figs. 5 to 8) reveal many details of the osteological teratology that accompanies the loss of the finger tips, the reduction of the toes, and the flattening of the thumb tips.

FIG. 1. HANDS OF III, 1, MALE, AGE 17 YEARS, SHOWING APICAL DYSTROPHY OF THE FOUR FINGERS OF EACH HAND

Middle and ring fingers of the left hand were connected by a fleshy web.

FIG. 2. FEET OF III, 1, SHOWING PARTIAL DYSTROPHY OF THE OUTER TOES, MOST MARKED IN DIGIT IV

The nail is wholly absent in IV, and vestigial in III and V. The third and fourth toes are webbed on both feet.

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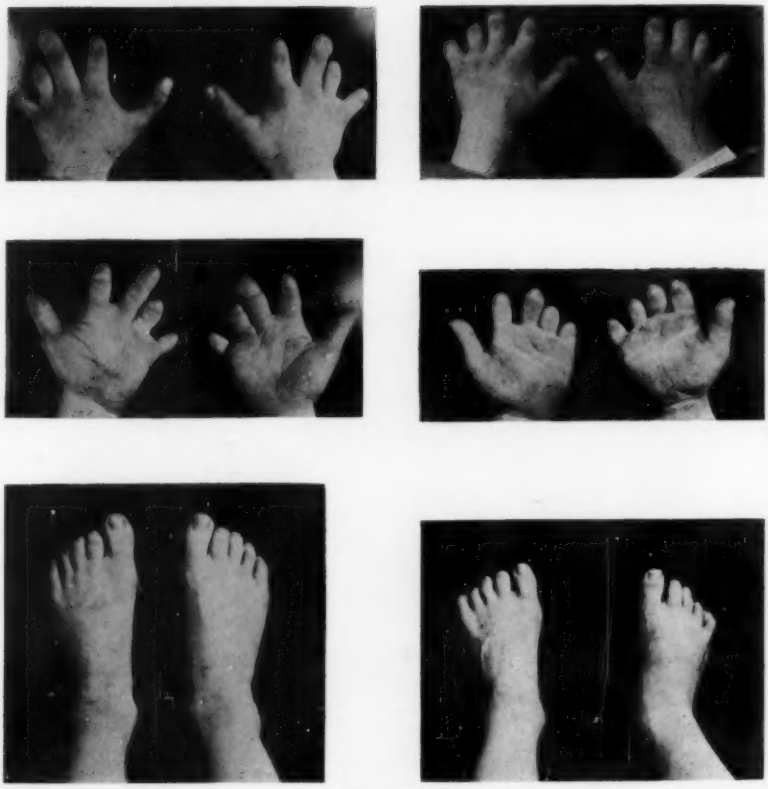

FIG. 3. HANDS AND FEET OF TWO OTHER BROTHERS, III, 3 (LEFT) AND III, 4 (RIGHT)

In amount of reduction of the digits there is a high degree of bilateral symmetry, but little vertical correspondence between hands and feet. In III, 3, digits III and IV of both hands were syndactylous.

FIG. 4. HANDS AND FEET OF FATHER, II, 1, SHOWING THE MORE MARKED REDUCTION OF THE OUTER DIGITS

There is a vestige of a nail in a deep pit on the left index finger, and the thumb nail is especially broad and thin, and partially doubled.

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FIG. 5. RADIOGRAPHS OF HANDS AND FEET OF III, 4. MALE, AGE 7 YEARS

In fingers II to V the distal phalanx is wholly absent, and the middle phalanx reduced to a rounded epiphysis in IV and V; a part of the middle shaft is seen in II and III. The terminal phalanx of the thumbs is definitely cleft and partly doubled. The terminal bone is missing from the four outer toes.

FIG. 6. RADIOGRAPH OF HANDS OF III, 3, MALE, AGE 12 YEARS

Fingers IV and V show no bones beyond the epiphysis of the middle phalanx. The index fingers show both elements of the middle phalanx. The third fingers lack only the shaft of the ungual phalanx. A medial thickening indicates incipient splitting of the terminal phalanx of the thumbs.

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FIG. 7. RADIOGRAPHS OF HANDS AND FEET OF III, 1, MALE, AGE 17 YEARS

From each of the fingers the distal phalanx and part or all of the middle shaft is missing. The terminal phalanx of each thumb is partly bifurcated, the open cleft or perforation showing clearly in the right, which is also distinctly longer than the left terminal phalanx. The four outer toes, whether outwardly shortened or not, lack the terminal phalanx, and IV lacks also the shaft of the middle phalanx.

FIG. 8. RADIOGRAPHS OF HANDS AND FEET OF II, 1, MALE, AGE 47 YEARS

The rough porous ungual expansion of fingers II and III is developed at the end of the middle shaft even though the nail may not be visibly developed. The terminal phalanx of each thumb is distinctly cleft and twinned at the tip.

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No reduction or peculiarity has been noted in the metacarpals (or metatarsals), or in the epiphysis or shaft of the proximal phalanx of any digit. Abnormalities are thus confined to the two outer phalanges, whose bony elements undergo reduction much more marked than those of fleshy parts and nails. In affected individuals even the toes (II-V) which appear entirely normal in exterior view and were always thought sound, have the bones obviously somewhat reduced, and in a way characteristic of dystrophy. Where slight abortion of the end of a digit is found the second phalanx is present throughout its length and usually normal, but the terminal one is *narrowed distally,* or even reduced to a mere *spherical* fragment, which evidently represents the epiphysis. In general in fingers and toes that are much shortened the distal (third) phalanx is entirely omitted, and the second is represented by a small rounded (epiphyseal) remnant, or by this together with a tapering conical, short or slender, densely ossified base of the second shaft, or even a whole second shaft complete to its distal extremity, which may bear a flaring porous ungual expansion as if for the support of a nail.

This expansion is, in our view, not at all diagnostic of the third phalanx, but is to be regarded as a functional modification, which may be induced on the shaft of any, even a lower phalanx—so long as it is outermost and terminal—by stimuli from the stump end of the finger. It may occur whether the nail is visible or not, and in fact a vestigial nail may be present without any trace of the third phalanx or the nail may be absent even when a part of the third phalanx is present.

Since the shafts first ossify early in the prenatal period and the epiphyseal cartilages not until one to five years of postnatal life (22), (32), (33), a final and positive determination of the homologies of the bones present in the abortive digits may be made from radiographs of affected extremities of fetuses, new-born infants, or very young children, where diaphyses show advanced ossification and epiphyses are still cartilaginous. Such ages were not available among our cases, but a comparison of Clarke's illustrations (7) of the defective hands of a mother and her infant son (showing deformities phenotypically very similar to ours, p. 196) confirms entirely the identifications we have made above (see p. 189). If this interpretation of parts is correct and epiphyses may occur and ossify progressively in the entire absence of their shafts, it suggests a far greater independence of these elements than has generally been assumed. In any case it seems certain that the epiphyses have nothing peculiar to do with this anomaly, which is strictly

congenital and has already been determined in development and is fully manifest long before the epiphyses begin to ossify.

It is impossible also to regard the missing or reduced phalanx in apical dystrophy as the second and the one retained as the third. Actually in both the toes and fingers the first reduction and loss that can be detected in the bony elements occurs at the apical end of the third phalanx and proceeds thence a variable distance basally; the elements disappear one after another from the tip of the digit towards the first knuckle. *There is no intimation in any case of a complete terminal along with a reduced middle phalanx.* The observations contradict at every point the view that a constant terminal phalanx resorbs or "assimilates" the middle or the middle and basal phalanges. The authors who have upheld this view (26) (7) either came before the time of X-rays (26), or neglected any study of the feet or of fingers having more than two phalanges (7). A proper analysis requires a good series showing the incipient stages of the reduction, and a realization that an ungual expansion is not diagnostic of the third phalanx. The apico-basal direction of the loss of elements (distal before middle phalanx, and shaft before epiphysis) appears to distinguish apical dystrophy from all of the various forms of the classical brachyphalangy (of Farabee (15), Drinkwater (11), (12), (13), *et al.*), in all of which the first reduction is seen in the epiphysis—usually of the middle phalanx.

OTHER CHARACTERS ASSOCIATED WITH APICAL DYSTROPHY

No individual in the family has fingers that are crooked ("bent") or stiff from ankylosis in any degree; flexion of the shortened fingers is of course, limited in extent, but is normal at each joint that is present, as the skin folds show. How the attachments of individual muscles and tendons are affected is not known. Despite the obvious handicap of stumpy nailless fingers in grasp and in delicate manipulations the affected individuals show an unexpected dexterity and adaptability in writing, throwing, tieing, and picking up small objects, and the adults are not prevented from entering and following successfully a wide variety of artisan occupations; they are not forced into unskilled labor, as Drinkwater's brachydactylous were (11). The stature of affected adults is not less on the average than that of their normal relatives; all the men are medium to tall, II 3 (Fig. 9) being 67.5 inches in height, II 1, 68.5 inches, and II 9, 72.5 inches. In fact there are no remarkable

physical or mental characters either in individuals or in the family as a whole, excepting the syndactyly and broadened thumbs with more or less split or double nails, which recur frequently, always associated with dystrophy.

Syndactyly. III 3 (Fig. 3) was born with partial fusion of fingers III and IV of both hands, the merely fleshy webs having been cut by the surgeon; this boy's toes (Fig. 3) are not webbed. His elder brother (III 1, Figs. 1 and 2) had webbing between digits III and IV of the left hand and of both the feet. The younger brother and their father are wholly free from even this mildest syndactyly, but two of the father's brothers have some webbing between toes, involving in II 9 digits IV and V, in II 3 digits II and III of both feet. The fact that no syndactyly is found elsewhere in normal or affected members, and is so asymmetrical and irregular when it does occur, suggests that it may be another, variable, expression of the gene controlling the abortion of the digits, rather than a separate character combined at random, or the effect of some modifying factor acting only in the presence of the defect. This view is also to be favored regarding split thumb nails.

Doubling of the thumbs. The thumbs tend in all cases to be abnormally stout and particularly broad and thinned at the tip, and possess wide, thin, brittle, and very slow-growing nails. The nails are often split partly or wholly in two, the line of bifurcation being marked by a longitudinal fold of skin running from the free end some distance down the middle of the nail. Such doubling is outwardly noticeable here and there in the family: in II 1, 3 and 7, and in III 1, 3, and 4. The splitting of the bone of the terminal phalanx of the thumb is more or less obvious in all of the thumbs radiographed, and would seem to be a regular, though variable, feature of the affliction. A real bifurcation of the whole end of the thumbs both externally and internally was noted by Cragg and Drinkwater (8) in their family with apical dystrophy, distributed sporadically and expressed irregularly, and was attributed by them, with many reservations and doubts, to a dominant modifying factor. Incidentally, Chilton (6) reports a family in which such a doubling affected the tips of all the hand digits and in which the outer phalanges were shortened, though nails were present.

These two associated characters illustrate a common experience in hand anomalies—the simultaneous occurrence and correlation in the same family and individual of two or more related defects. Both syndactyly and double thumbs may occur independently of the dys-

trophy, but it would seem almost infinitely improbable, according to laws of chance, that this family should be triply heterozygous for three genetic digital abnormalities, and should harbor separate genes for anomalies as rare in the population as dystrophy, split thumbs and syndactyly. If the webbing and doubling are controlled by separate genes these must either be very closely linked with the dystrophy gene in all of the several families where they are associated, or be modifying genes, very common, if not almost omnipresent, in the population.

It might be noted that the nailless condition described above is totally different from that one, also dominant, associated with a more general ectodermal dystrophy of hair, nails, and teeth, e. g. Jacobsen (21). Such failure of development of hair and teeth is here entirely lacking, for these structures are normal, and the nails themselves are absent only when their sites have been removed by other causes. Nails may be said to be genetically present and normal, and the essential factor in apical dystrophy is not one specifically affecting either nails or finger prints but one causing the earlier dystrophy of the finger tips.

Finger print patterns. The dystrophic finger stumps exhibit a series of alterations apparently unique in dermatoglyphic studies, and contributing additional proof that the terminal, not the middle, phalanges are missing. Thumb and finger prints were taken from both hands of six individuals with shortened digits: II 1, 7, and 9, and III 1, 3, and 4. The patterns, deficient as they are, are difficult to print as a whole because they are displaced to the extreme end of the finger, and this surface is seldom smoothly rounded but is usually very irregular and scarred.

All twelve thumbs, even the exceedingly thin, broad ones, carried unremarkable ulnar loops. The fingers, however, show few resemblances to the described pattern types. Most of them totally lack triradii and these, if present, occur high up on the sides of the extreme tip of the finger. In general the most characteristic direction of ridges is approximately transverse like those normally present on the skin of the volar side of the first and second phalanges. The finger "ball" is commonly much elevated, blunt round or pointed, and around this ball the transverse lines bend to form a series of concentric circles, the center of which lies at the highest point of the elevation. Such a circle pattern found on 36 fingers of the 12 hands bears close resemblance to, if it is not identical with, Bonnevie's (5) theoretical type (1929, Fig. 2) of a whorl with a continuous distribution (with ridges overspreading

the whole end of the finger) but which she never found realized in her extensive experience.

The longer fingers, especially those with a rudiment of a nail, may show transitions towards the common patterns. Low incipient arches are found on both index fingers of II 1, and both middle fingers of III 3. Recognizable radial loops occur on the left index finger of II 7 and the right ring finger of III 4; ulnar loops on both little fingers of III 4. The central lines of a circle are occasionally reversed or flattened, reminding one of a whorl or loop.

THE FAMILY HISTORY OF THE DEFORMITY

The three brothers already mentioned have or have had five first cousins, four uncles, one aunt, a father and a grandfather similarly affected (Fig. 9). The grandfather, now dead, came as a youth from the south of England to Toronto, where he married a native-born normal woman. Since he seldom spoke of his old country relatives and apparently recalled them indistinctly, there is left in the present family no record or tradition of the earlier occurrence of the defect in his family. Of his seven children, five sons survive and two daughters have died. All of the sons and one daughter (who died in infancy) suffered from birth from the terminally shortened fingers. The five sons married normal wives, are local residents, and were visited. Of the total of 16 children born to these five marriages eight possess the defect, five of these being boys and three girls. One of the normal girls is married and has a normal son. This is the only marriage of normals recorded in the family.

A simple mendelian interpretation of the data is afforded on the assumption that this, like most other kinds of short fingers, is a monofactorial dominant character, transmitted in each generation by affected heterozygotes, *Dd*, in marriages with normals, *dd*, to about half of their children. Thus, while the original marriage of *Dd* × *dd* produced 6 affected to only 1 normal, a marked excess of abnormals, the five similar marriages of the next generation produced exactly 8 affected and 8 normal children. The total of 14 abnormals to 9 normals is not widely divergent from the expected equality. In this family every abnormal has come from one abnormal parent marrying a normal, and every such abnormal, except one, an only child, has one or more normal sibs. Up to the present abnormal males have transmitted the defect directly to both sons and daughters; there seems no warrant for considering the

excess of affected males in the second generation as indicating more than an especially one-sided sex ratio in that one family (see Fig. 9). It is likely that the transmission is by males or females to males or females impartially. The observation of Mohr and Wriedt (27) of a possible homozygous dominant lethal in brachyphalangy arouses interest

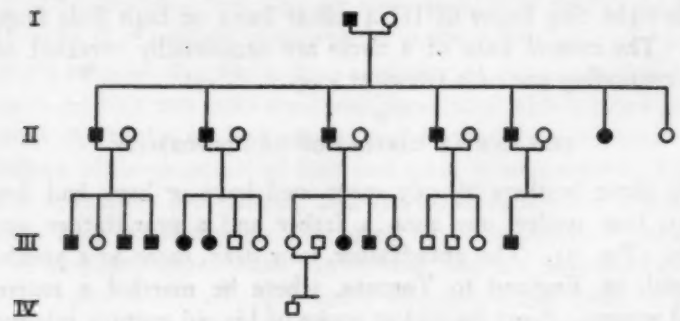

FIG. 9. COMPLETE PEDIGREE CHART, SHOWING THE MEMBERS OF A FAMILY WITH SHORTENED NAILLESS FINGERS (SHADED), AND THOSE UNAFFECTED (UNSHADED)

as to the nature and viability of an individual homozygous for apical dystrophy, but the short family history has not furnished a case of marriage of two affected heterozygotes in whose progeny such a person might appear.

OTHER INSTANCES OF A SIMILAR DEFORMITY

The earliest cases of hands with fingers shortened by loss of the tips are quite certainly of an entirely different nature, but are interesting because of certain striking likenesses to the hereditary type. From the walls of grottoes in the Pyrenees l'Abbé Breuil described and depicted "silhouettes of the hand in black and red produced by pressing the hand against the limestone wall and covering the surrounding surface with color. It would appear that the fingers were mutilated or cut off at the middle joint, because one, two, three, or four of the fingers are wanting, but the thumb is never mutilated. This mutilation of the hands may be compared with similar practices prevailing among some Australian tribes" (29). Under the term perophalangy Pol (30) collected a number of isolated cases showing fetal amputations of different grades; some of them are bilateral and the amniotic strands sometimes cut off

similar portions of two hands and two feet. By general agreement these cases are distinct and atypical, because the defects are irregular, asymmetrical (though bilateral) and entirely non-familial.

Unquestionably hereditary, similar phenotypically, and probably in large part genetically identical with ours are several early family records from the literature of the last century or more.

1. A mother and two of her children were inspected by a surgeon at Uxbridge (Kellie (23), 1808): "on examining the hands, the thumbs only appeared perfect; instead of fingers they had only the first phalanx of each finger, and the first and second of the ring finger of the left hand. The fingers had no nails." The mother stated that the same deformity affected her other children (who were not examined), and had been in her direct line of ancestors for each of the nine preceding generations uninterruptedly, and "that the women only had the misfortune of entailing this defect on their offspring," though they also, but "not often," had normal children.

2. Mackinder (26) (1857) reported a deficiency of finger tips transmitted through six generations. Some variety of somatic types was manifest, but in general there was a symmetrical shortening of fingers II to V to one or two phalanges with a corresponding lack or abortive condition of the nails. The study—without X-rays—seemed to him to indicate the shortening or complete absence of the second row of phalanges in all these fingers, and a further shortening or absence of the third row in the ring and little fingers. A flattening of the thumb was noted. The feet were usually similarly involved to a greater or less degree, digit IV being oftenest monophalangic. One man had a fleshy web between toes IV and V.

3. In a Cincinnati family known to Dr. William Carson (Neilson (28), 1889) a patient "presented a deformity consisting in the absence of a distal phalanx, and part of the second, from all of the fingers of both hands, the thumbs being normal." Whether the feet were abnormal is not mentioned, and no detailed descriptions or photographs accompany the note. The hand defect was stated to have been observed in the family continuously for over a hundred years. In the first generation of the pedigree given there were five affected out of eleven children. One of the affected males by a normal woman had three defective out of five surviving children who could be classified. One of these three abnormals was the mother of the probandus, an only child, who gave the history.

4. In a family of Virginia negroes Smith (35) (1904) traced a congenital digital malformation for three generations. A grandfather transmitted it to a daughter, who had four normal sibs, and she in turn transmitted it to four daughters and two sons, who had three normal sibs. The grandfather had normal thumbs, but only two phalanges per finger on each hand and a small nail on the index but none on the other fingers. As a general rule in this family the finger stumps were abnormally broad, and one phalanx was missing from each finger, the terminal phalanx being rudimentary on IV, and usually on III and V as well: small nails were borne frequently on II, sometimes on III and in one case on digit IV.

5. Bateson (2) (1909) cites another probable case: "a correspondent has sent me a pedigree of a family in which the fingers are almost entirely aborted. The descent was that of an ordinary dominant. The families of the affected add up to 19 affected and 16 unaffected. The children of the unaffected were normal in all cases."

6. Kidd (24) (1910, seen in abstract (30)) described a man with hands and feet symmetrically biphalangic throughout, the nails being absent from fingers II to IV, but not from the fifth finger, which was "bent," or from the toes, two of which, II and III, were connected by a web of flesh. The dystrophy was traced back through two more generations continuously and onward to his child.

7. Drey (10) (1912) gives details of nine cases, four male and five female, in three generations. The defect was a somewhat different, but evidently allied, digital shortening, in which the distal phalanx and nails were lacking from some of the fingers, especially IV and III, of both or only one of the hands while the feet seemed normal. The defect was an irregular dominant. A stiff index finger, due to a synostosis of phalanges, occurred at random in this family, both in those with normal and in those with abnormal finger tips.

8. Clarke (7) (1915) presents the first illustrated account of a rather similar familial condition. The family chart shows 10 abnormals produced without skips in 4 generations, but normals are unfortunately omitted. The abnormality is present at birth, is transmitted by both sexes, and "seems to affect the two first born in each family irrespective of sex." The patient was a maternity case, who already had had two affected children and gave birth to another affected son by a second father. Sketches and radiographs of the hands of the mother and the new born son show normal thumbs (with the bones apparently, though

incipiently, split) and shortened fingers, the tips of which "are rounded off without any indication of a nail, excepting a minute vestige on the left index finger" of the mother. On digits IV and V the middle and terminal phalanges are absent and in II and III, the longest fingers, "the middle phalanx is seen to be absent, and not the terminal phalanx as might be expected from the appearance of the hand." There was evidently a fleshy webbing between III and IV of the mother's right hand. In view of our discussion on page 189 the osteology of the infant's hand is interesting. Only the shafts, not the epiphyses, of the phalanges show ossification; the shafts are of the same shape and in the same position in mother and child, but the ball-like element at the base of the middle phalanx of IV and V in the mother (and in the very short fingers in all our cases) which we have identified as the epiphysis of that phalanx is wholly missing in the infant.

9. The largest and best described, as well as the most degenerate set of cases from the literature is that from an old Lincolnshire family with 27 defectives in five continuous generations (Cragg and Drinkwater (8), 1916). The anomaly they describe is again undoubtedly identical with that in our cases. There are the same somatic manifestations, the same symmetry, the regular mendelian dominance with excess of abnormals, and the same scattering among the affected of those with definitely flattened and bifurcated thumb tips. But their cases were more uniformly defective in shortness of fingers and in splitting of the thumbs, than ours, which show many more transitions towards the normal in both these respects. They mentioned no syndactyly in any case, and did not illustrate and seldom even actually inspected the condition of the feet.

Yet between this family and ours no recent connection seems likely; they hail from different parts of Britain, and have no single surname or favorite given names in common. In fact there is at present no definite suggestion of a community of origin of any two of the families described, though, with the possible exception of Drey's, they exhibit essentially the same defects. However, the cases are all from northwestern Europe, and their distribution in the English-speaking nations is such as could be accounted for on the assumption of the dissemination of the defect during the last two centuries from Scotland or north of England over England, to North America with white settlers, and finally into the negro family.

Taking the families collectively and comparatively as the best ways of arriving at a proper understanding of the variability and the genetic behavior of the apical dystrophy, it is seen that:

The fingers vary all the way from those with one (and a fraction?) phalanges to those of nearly full normal length. Digits II and III are commonly reported longest in families 1, 2, 8, and 10, IV and V in family 7. The perfection of the finger nails varies in the same way, declining from the radial digits to the ulnar ones, or else grading down in the order I, II, III, V, IV. The toes and toe nails are less reduced, but imperfections apparently increase from digit to digit in the order last mentioned above. Radiographs of the feet seem not to have been published hitherto.

The persisting phalanges have been described as the proximal and middle rows, with the distal row omitted (families 1, 3, 6, 8, and 10), but the middle row was regarded as missing in families 2 and 7 (p. 190).

The sex distribution of the cases, so far as they have been mentioned (Table 1) is 37 males: 49 females. Apical dystrophy appears to be one of the few defects and infirmities which are commoner in the female sex. To all appearances either sex is about equally likely to receive the defect from either sex, and to pass it on to either sex.

The abnormality is clearly a simple mendelian dominant. Normals segregating out in the families have only normal children. The heterozygote is distinctly abnormal, and there are no reported instances of skipping of generations between abnormals. The progeny of affected, *Dd* × normal total 91 abnormal: 73 normal; the excess of abnormals is concentrated in families 9 and 10, which are at once the largest, the latest, and the most thoroughly studied, and are, it is believed, accurately and completely classified in every individual in the pedigrees.

Of the associated characters syndactyly is mentioned in families 2, 6, and 10, and is clearly shown in the figures accompanying 8. Flat or split thumbs occur in families 2, 8, 9, and 10, though again they are not mentioned in family 8. Both of the defects will probably prove quite regular features of apical dystrophy; they may well have been present and overlooked in most of the earlier families, before photography became the vogue and drew attention to details. There are also two families with one or more cases of an ankylosis of phalanges, which may be only incidental, producing bent little finger in one family, and stiff index fingers in the other.

TABLE 1

Summary of Cases of Apical Dystrophy

FAMILY	AUTHORITY	YEAR	SEX OF AFFECTED		MENDELIAN RATIO IN PROGENY FROM:			ASSOCIATED DEFECTS FOUND
					aff. × norm.		norm. × norm.	
			♂	♀	aff.	N.		
1.	Kellie	1808	—	1	—	—	——	none mentioned
2.	Mackinder	1857	3	8	8	9	——	syndactyly flat thumb
3.	Carson	1889	2	1	9	8	——	none mentioned
4.	Smith	1904	3	5	7	7	——	none mentioned
5.	Bateson	1909	—	—	19	16	all normal	none mentioned
6.	Kidd	1910	1	5	—	—	——	syndactyly bent V finger
7.	Drey	1912	4	5	9	7	all normal	stiff II finger
8.	Clarke	1915	3	3	—	—	——	syndactyly split thumb
9.	Cragg and Drinkwater	1916	10	17	25	17	all normal	split thumb
10.	MacArthur and McCullough	1932	11	4	14	9	normal	syndactyly split thumb
			37 ♂	: 49 ♀	91 aff.	: 73 norm.	all normal	

Having in mind the variations shown in this series of families, we have sought for preferable alternative terms for the defect, and have rejected most of those suggested as inappropriate for one reason or another: "deficiency of fingers" (26), "hereditary absence of phalanges" (8), distal aphalangy, and hypophalangy, because no phalanx, even the distal one, need be wholly missing; and nailless or anonychial brachydactyly or hypophalangy (30), because the longer toes and fingers bear nails. More descriptive and fitting anatomically would be perodactyly, for mutilated fingers (30), hemidactyly (analogous to St. Hilaire's hemimely), for imperfect stumps of fingers, or ectrodactyly, for abortive fingers. On the whole apical dystrophy of the digits seems

to cover least objectionably the multiple abnormalities of the defect, and best to convey the impression that they arise developmentally and physiologically from a common underlying cause.

THE PATHOGENESIS OF APICAL DYSTROPHY

For years the prevailing theory of the etiology of hand defects has been that they are acquired from causes, mechanical or chemical, acting through the environment, before, at, or after birth. Plausible and sufficient explanation of isolated cases of short fingers is often afforded by appeal to amputations by the cord or amniotic bands, localized trauma, localized joint or bone infections, abnormal uterine environment due to crowding, pressure, etc. But such causes can hardly be held responsible in the typical cases, showing the highly symmetrical nature of the defect, its familial repetition generation after generation, and its sharp segregation among sibs. To explain these characteristics most reasonably it is necessary to postulate a regular distribution of internal inherited predispositions to the defect which rest finally with specific, abnormal, mutated genes.

Since apical dystrophy is prenatal in onset, one may seek suggestions from embryology and attempt to trace its genesis to earlier ontogenetic stages and to explain by some common cause the shortening of the fingers, the comparative immunity of the thumbs, and the frequent association with webbing of certain digits and doubling of the thumb ends.

The clues (22), (34) lead to a time early in the second month of fetal life when, on the hand and foot plates, the blastemas of the five digits are being grooved off. These are at first united by a web, from and beyond which the digits grow out and separate from each other. It is probably significant that even at this early stage the widest separation is that of the thumbs or great toes (on the radial or tibial side) from the other (ulnar or fibular) digits, the chief plane of symmetry in the dichotomy falling between digits I and II, and being more distinct in the hand than in the foot.

The phalanges differentiate in serial order, the basal row first, followed by the middle and then the terminal row. In the 20 mm. embryo, according to Lewis (22), each digit has just two phalangeal cartilages, indicating that the thumb, developing from the anterior part of the plate, is relatively complete, while the remaining fingers have still to differentiate their terminal cartilages and complete their development

in a brief but comparatively rapid growth, during which the nails and finger print patterns begin to appear and the cartilages to ossify.

The first of the primary ossification centers in the digits arises at the distal extremity of the shaft of the ungual phalanx; and again the thumb is distinctly in advance of the fingers. In fact the distal shaft of the thumb is the first bone in the hand to ossify, and this phalanx is further peculiar in that it is first laid down in connective tissue.

These facts suggest reasons for the differences in the pathology in the thumb and fingers. There is, moreover, a stage in development, when, simultaneously: certain fingers or toes are yet connected by webs, the fingers and outer toes are about to develop their terminal portions, and the thumb is differentiating its terminal phalanx. A speculative picture of the development of apical dystrophy may be drawn, assuming that the action of the controlling dominant gene is highly localized in time and place, and is of the nature of an inhibiting influence producing a partial arrest of development in the digits during this brief period. The most actively growing and rapidly developing parts are known to be most sensitive to arresting influences and most subject to retardation.

It has been experimentally demonstrated by Bagg and Little, Bagg (1), and Bean (3), that a number of hereditary (but recessive), congenital abnormalities of the extremities (including hypodactyly, amputation, syndactyly, and polydactyly) originally produced by X-rays in mice, first show in the embryo as localized lesions, themselves caused by prior, similarly localized, extravasations of lymph and blood toward the end of the developing extremities. In the case of apical dystrophy of the digits similar immediate causes, vascular, trophic, or trophoneuritic, might be operative, and it would be expected that the earlier and stronger the gene action, the more marked would be the shortening of the fingers and the failure of the flesh, nails, papillary patterns and bones of their outer portions, the more frequent the persistence of webbing between the digits, and the more likely and conspicuous the splitting or twinning of the thumb ends.

A functional developmental view of the specific gene action, varying in time, exact localization and extent, may help explain the frequent "syngenetic" association of various kinds of defects in the same member, and the considerable latitude of expression of the dystrophy, which is evident from finger to finger, limb to limb, and person to person. Differences in the severity of the dystrophy from family to family, however, are probably best accounted for genetically, by differences in accompanying genes or in quantities of the controlling gene.

APICAL DYSTROPHY CONTRASTED WITH BRACHYPHALANGY

The classical types of short fingers are the ones commonly described and illustrated in the text books. Much difference of opinion exists as to the most fitting broad and generic term to include all their numerous and varied forms (19). Many authors (27), (20) contend justly that Pfitzner's "brachyphalangy" is more precise and descriptive than Leboucq's earlier "brachydactyly," which has been adopted by Drinkwater (11) and popularized by Bateson (2), since the shortening of the digits is seldom, if ever, uniform along their length, but, as radiographs prove, is rather definitely confined to some one particular row of phalanges, which alone is shortened or omitted.

The features that are least variable and most characteristic in brachyphalangy have been determined by the comparative studies of such authors as Lewis (25), Drinkwater (11), (12), (13), Mohr and Wriedt (27), Pol (30), Esau (14), and Birkenfeld (4). With these in mind it is at once apparent that the two chief types of shortened fingers belong to different groups. Though they share certain resemblances, they contrast sharply in the time and nature of their manifestations, and in the seat and essential cause of the disturbances involved.

Like apical dystrophy, brachyphalangy affects more females than males (4), and post-axial digits more than preaxial ones (30), (31). They are not to be differentiated by their mendelian behavior, since both are simple mendelian dominants, though the dominance seems less regular in brachyphalangy.

Genetic evidence of their distinctness may sometime be provided by intermarriages between two individuals, one with apical dystrophy and the other with brachyphalangy; or by demonstration of different linkage relations of the controlling genes. But it hardly seems necessary to await such studies to separate them, since they are very different in somatic expression and in origin and development. A number of the differentiating characters are summarized in Table 2.

By general agreement the seat of primary disturbance in brachyphalangy is in the skeletal system, and restricted to certain cartilages and bones, most commonly the metacarpals (or metatarsals), or the middle phalanx of the digits, or the terminal phalanx of the thumb. Of importance in understanding brachyphalangy, but not apical dystrophy, are the secondary ossification centers in the epiphyses; these normally begin to appear only towards the third year after birth and continue to ossify and lengthen the bones until they join the shafts at about 18 to

20 years (earlier in females than in males, and in the hands than in the feet). In brachyphalangy a hereditary precocious closing of the epiphyseal line, of the middle phalanx, for instance, stops its elongation prematurely while the other phalanges continue their growth. Pol (30),

TABLE 2

Differentiating Characters of the Two Chief Types of Short Fingers

	BRACHYPHALANGY	APICAL DYSTROPHY OF DIGIT
Age of onset	Chiefly postnatal, becoming conspicuous only during childhood growth (3-10 yrs.)	Fully manifest at birth, probably determined finally as early as the second month of fetal life.
Somatic expression	Feet more degenerate than hands.	Hands more seriously defective than feet.
	Digit I often shortened, as well as II-V.	Shortening involves only digits II-V.
	Shortening confined to one row of phalanges; usually the penultimate phalanx is wholly absent or reduced to a broad cubical or rectangular mass, often fused to the ungual phalanx.	Reduction of the digit from the tip a variable distance toward the base, involving first the ungual and then the middle phalanx.
Essential feature	A localized disturbance of bone growth, due to absence, or premature ossification of the epiphysis, which fuses with its shaft, causing an early cessation of growth in length.	An embryonic failure of development of the tip of digit—bone and nail deficiencies being secondary and incidental.
Associated characters	Stature markedly reduced by early cessation of growth of limb bones in length.	Stature not reduced.
	Nails and papillary patterns normal.	Nails and finger print patterns incidentally involved.
	Frequent association with ankylosis, hyperphalangy, syndactyly, but not with split thumbs.	Frequent association with flat bifurcated thumbs and fleshy webbing between certain digits.

dissenting mildly, claims that brachyphalangy must begin in early fetal life when the shafts are ossifying, but the fact remains unassailable that in brachyphalangy the short digits, like the short stature, are not usually detectable until a certain age is reached (11), (25), (4).

Classification of the Forms of Short Fingers (Brachydactyly)

Though apical dystrophy might be considered an entity, such is certainly not true of brachyphalangy, of which a much wider range of fairly distinct types occurs in different families. The cited reviews (4), (12), (14), (20), (25), (27), (30), (31) of an extensive literature on hereditary short fingers and toes show the great variety of reductions and combinations of reductions that occur (14), the numerical preponderance of a few of these (30), (20), and the possibility of grouping them in a graded series of subtypes. The shortening may involve one, or more, or all the digits of one or both hands or feet, or of both hands and feet; and one, two, or all three of the phalanges of a digit. But by far the most frequent segments affected are: (1) the metacarpals (metatarsals), (2) the middle phalanx of one or more digits (30), or (3) the terminal phalanx of the thumbs (20). In the first case (brachymetapody (30)) the digit is really not shortened, but may appear so, because of the reduction in length of a metacarpal or metatarsal. To the large second group (brachymesophalangy (30)) falls the great bulk of the better known types and subtypes of "brachydactyly," "minor brachydactyly," "klinodactyly," etc. The third group (brachytelophalangy (30)) consists chiefly of cases of hereditary short thumbs (18), (20). Wherever the shortening is localized, the etiology is practically the same, being displayed in the epiphyseal insufficiency mentioned above. Pol (30) has pointed out that the brachyphalangies seem to be extreme exaggerations of normal tendencies, since the particular phalanges and digits most frequently affected are also the ones which, in normal hands, most frequently have imperfect or no epiphyses.

The main types and subtypes of hereditary short fingers may be provisionally classified on the basis of their most distinctive anatomical characters and the time and nature of their development. The intention is, not to classify all known types, but to begin to simplify the basic concepts, point out various synonymies and relationships, and reduce the excessive and deplorable duplication of terms for apparently homologous defects that now burdens the literature.

1. Shortening prenatal, involving digits II to IV, especially on the hand, the cause acting apico-basally in direction, inhibiting development of varying amounts of the terminal portions of flesh, nails and bones.

(Apical dystrophy of the digits, etc. (8), (26), (30))

2. Shortening typically not fully apparent at birth, but developing postnatally during growth, due to the precocious ossification or absence

of certain (rows of) epiphyses and the consequent failure of normal elongation of these bones (brachyphalangy in the broadest sense); the disproportionate reduction affecting chiefly:

A. Metacarpals or metatarsals (brachymetapody (30))

B. The middle (penultimate) phalanges (brachymesophalangy (30))

1. Shortening more wide-spread, involving other bones (and possibly allied to the endocrinal disturbances in chondrodystrophy (4)), stature being decreased by the early cessation of growth of the long bones, and the digits being reduced by the shortening or virtual absence in the adult of the middle phalanx of II-V and the basal phalanx of I, which may be absorbed by the ungual phalanx.
   a. Reduction marked, the phalanges being absent, or short and malformed, and fused to the base of the terminal phalanges. (hypophalangy (15), assimilation hypophalangy (30), brachydactyly (11), biphalangy (30)).
   b. Reduction less marked, penultimate phalanges being present as separate bones, not ankylosed to the terminal phalanges (minor brachydactyly (12), etc. (30)).
2. Shortening restricted to the middle phalanges of certain digits, especially V and II, stature being normal.
   a. Digits II-V (toes only) without a middle phalanx (9).
   b. Digits II of hands and feet, middle phalanx showing
      (1) marked reduction (B′ type (27)).
      (2) slight reduction (B type (27)).
   c. Digits V, II, or IV, of the hand, showing a slight and asymmetrical shortening on the mesial side (klinodactyly (4), (30), crooked little fingers (19)).

C. Terminal phalanges (brachytelophalangy (30))

1. Shortening confined to the thumbs, which are broad or clubbed (brachymegalodactyly (18), etc. (20)).

There are certainly no specific genes controlling the deficiency of nails and finger print patterns, and probably none determining bifurcate thumbs and webbing. The whole complex of characters may best be regarded as a genetic entity under common monofactorial control. The gene action may be a localized inhibitory one timed to occur when the digits are separating and their tips actively growing out from the hand and foot plates.

The classical types of *brachyphalangy* ("brachydactyly," etc.) are sharply differentiated from *apical dystrophy* in time of onset, in localization of effect, and evidently in essential cause. The reduction in apical dystrophy is fully manifest at birth, is not specifically localized in epiphyses, and acts apico-basally in direction, affecting the shaft before the epiphysis and the terminal phalanx before the middle one.

Apical dystrophy is a monofactorial dominant character, in this and other families transmitted entirely by marriages of the Aa × aa type to progeny approximately one-half affected and one-half normal, though abnormals are often in excess.

LIST OF REFERENCES

1. Bagg, H. J. Hereditary abnormalities of the limbs, their origin and transmission. II. A morphological study with special reference to the etiology of club-feet, syndactylism, hypodactylism, and congenital amputation in the descendants of x-rayed mice. *Am. Jl. Anat.*, 43: 167-219.
2. Bateson, W. 1909. Mendel's Principles of Heredity. 2nd ed. Cambridge Univ. Press, p. 216.
3. Bean, A. M. A morphological analysis of the foot abnormalities occurring in the descendants of x-rayed mice. *Am. Jl. Anat.*, 43: 221-246.
4. Birkenfeld, W. 1928. Über die Erblichkeit der Brachyphalangie. *Arch. f. Klin. Chir.*, 151: 611-631.
5. Bonnevie, K. 1929-1931. Zur Mechanik der Papillarmusterbildung I-III. cf. *Arch. f. Entw. Mech.*, 117: 384-420.
6. Chilton, N. 1927. Hereditary deformities of the fingers. *Br. Med. Jl.*, 1927, p. 15.
7. Clarke, D. S. 1915. Congenital hereditary absence of some of the digital phalanges. *Br. Med. Jl.*, 1915, p. 255.
8. Cragg, E., and H. Drinkwater. 1916. Hereditary absence of phalanges through five generations. *Jl. Gen.*, 6: 81-89.
9. Cutter, R. T., and W. M. Nelson. 1931. A short-toed family. *Jl. Heredity*, 22: 163-166.
10. Drey, J. 1912. Hereditäre Brachydactylie kombiniert mit Ankylose einzelne Fingergelenke. *Zeitschr. f. Kinderheilk.*, 4: 551-561.
11. Drinkwater, H. 1908. An account of a brachydactylous family. *Proc. Roy. Soc. Edinb.*, 27: 35-57.
12. ———. 1912. Minor brachydactyly. *Jl. Gen.*, 2: 21-40; and 3: 217-220.
13. ———. 1915. A second brachydactylous family. *Jl. Gen.*, 4: 323-340.
14. Esau. 1924. Die Brachyphalangie—eine erbliche Missbildung. *Arch. f. Klin. Chir.*, 130: 786-792.
15. Farabee, W. C. 1905. Inheritance of digital malformations in man. *Papers Peabody Mus., Harvard Univ.*, III, 3: 69.

16. GILLETTE, C. P. 1931. An inheritable defect of the human hand. *Jl. Heredity*, 22: 189-190.

17. HALL, G. A. M. 1928. Hereditary brachydactylism. *Annals of Eugenics*, III, pts. III and IV.

18. HEFNER, R. A. 1924. Short thumbs (brachymegalodactyly). *Jl. Heredity*, 15.

19. ———. 1929. Inheritance of crooked little fingers. *Jl. Heredity*, 20: 395-398.

20. HOFFMAN, H. 1928. Zur Brachyphalangie des Daumens. *Klin. Wochenschrift*, 72: 2155-7.

21. JACOBSEN. 1928. Hereditary dystrophy of the hair and nails. *Jl. Am. Med. Ass.*, Mar. 3, 1928.

22. KEIBEL and MALL. 1910. Human Embryology, vol. I, pp. 388-390. Lippincott.

23. KELLIE, DR. 1808. Letter Mr. L. to Dr. Kellie. *Edinb. Med. Jl.*, 4:252.

24. KIDD. 1910. Abnormal phalanges in a human hand: a case of brachydactyly. *Jl. Anat. and Psychol.*, 64:

25. LEWIS, THOS. 1907. Brachydactylism. Treasury of Human Inheritance. Univ. of London, Memoir no. VI.

26. MACKINDER, D. 1857. Deficiency of fingers transmitted through six generations. *Br. Med. Jl.*, 1857: 845-6.

27. MOHR, O. L., and CH. WREIDT. 1919. A new type of hereditary brachyphalangy in man. *Carnegie Inst. Publ., Washington.* No. 295.

28. NEILSON, T. R. 1889. Congenital abnormalities of the extremities. Keating's Enc. of Diseases of Children. *Philadelphia.* III: 935-936.

29. OSBORN, H. F. 1916. Men of the Old Stone Age.

30. POL. 1921. Brachydactyly- klinodactyly-hyperphalangie und ihre Grundlagen. *Virchows Archiv*, 229: 388-530.

31. PFITZNER. 1898. Über Brachyphalangie und Verwandtes. *Verhandl. Anat. Ges.*, 12: 17-20.

32. PRYOR, J. W. 1905. Development of the bones of the hand as shown by the x-ray method. *Bull. State Coll. Kentucky*, Series 3, No. 4.

33. ———. 1906. Ossification of the epiphyses of the hand—x-ray method. *Bull. State Coll. Kentucky*, Series 3, No. 5.

34. ROBINSON, E. 1915. Cunningham's Text-book of Anatomy, 4th ed., pp. 78 and 227-8.

35. SMITH, H. M. 1904. Congenital digital malformations in negroes. *Amer. Anth.*, N. S. 6: 579-580.

BIOMETRICAL STUDIES IN PSYCHIATRY

VII. TUBERCULOSIS, SYPHILIS AND CANCER

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The preceding paper of this series dealt with the general problem of population and death, thus introducing the question of the significance of certain diseases in the morbidity and mortality rates of the hospital. The figures in this paper are based upon the data obtained at necropsy, and both deaths and death rates are calculated upon the basis of this necropsy material. The main objection to this procedure is that not all patients are submitted to post mortem examination; on the other hand, the high percentage of error in the clinical diagnosis in psychotic patients renders such figures as might be calculated from the death certificates rather less reliable than those that can be presented from necropsy material. Knowing the number of deaths and the percentage submitted to necropsy, it is easy to calculate the probable number of cases of a given disease that have occurred. The accuracy of the figures will depend upon the necropsy percentage and upon the random sampling of the necropsy material as well as upon the reliability and completeness of the necropsy. Since good conditions have prevailed for the past 45 years in this regard, it is believed that trends in mortality rates will truly represent the actual conditions, at least for the psychotic population.

In taking up the three diseases, tuberculosis, syphilis and cancer, I have chosen diseases that are for the most part clear-cut and, at least in the first two, are of known etiology. Moreover both syphilis and tuberculosis are important factors in the high mortality rates of the psychotic population, while malignant tumors appear to be increasing in importance. Interesting data are available for comparison of the immunity and resistance of various psychological types to these conditions, thus leading to a better evaluation of the constitutional factor, and lastly some conclusions are drawn concerning the interaction of these

three diseases upon each other. The cases are believed to be sufficiently numerous to lend themselves to statistical handling, and the criteria for their recognition are much better defined than for such conditions as chronic myocarditis and coronary thrombosis. Of considerable interest is the mortality trend in these diseases in the 45 years for which data are available.

Tuberculosis

A post mortem diagnosis of active tuberculosis has been made in 960 cases of the 5600, an incidence of something over 17 per cent. In the great majority of cases this affected the lungs, but occasionally the lesions were only in the lymphatic, urogenital or osseous systems. The clinical and pathologic peculiarities of this disease are well known, and certain phases have been treated in other papers of this series.[1] As ordinarily understood, tuberculosis is a disease of youth and the majority of cases fall in the younger decades. Nevertheless, the death rate from tuberculosis rises steadily with increasing years and is at its highest in the most advanced years. I have performed a necropsy upon an individual 97 years old, who showed advanced caseous and ulcerative tuberculosis of the lungs. Mortality statistics for the registration area bear this out for the male, the high point of 170 per 100,000 coming at the age of 70, although in the case of females there is an early peak at 22 and a late one at 70. In our material, likewise, the greater susceptibility of the colored race is apparent, their death rate running from two to four times that for the white race. The greater susceptibility of the psychotic population to tuberculosis is very manifest, since the rates are something like ten times as high in the autopsied population of this hospital as in the population at large.

I have pointed out previously the great susceptibility of the schizophrenic individual to tuberculosis, 317 per 1000, three times as great as all the other types combined. In other words, seven out of eight individuals with tuberculosis belong to the schizoid group. It is a common saying that this susceptibility is due to the habits of life of the schizophrenic individual, his tendency to keep to himself, to sit hunched up in a chair, to be unclean and untidy, and to expectorate promiscuously.

[1] Freeman, Walter: Biometrical Studies in Psychiatry. III. The Chances of Death, *St. Elis. Hosp. Bull.* No. 6. 1930. *Idem.* V. Psychologic-Nosologic Correlations, *Same,* To be published.

The regression of the schizophrenic individual is undoubtedly deep, but his deterioration is believed to be slower and of a somewhat different nature than that of the confirmed epileptic, and the epileptics come to resemble in many outward respects the patient with long-standing dementia precox. Yet the tuberculosis rate for the epileptic is only a fifth of that for the schizoid individual. The peculiar bodily conformation of the schizoid individual, tending to the asthenic habitus, is also advanced as a reason. Looking at it from the constitutional standpoint, however, there would seem to be an underlying factor that determines on the physical side the leptosome habitus, on the chemical side the lowered metabolic and oxidative force with underactivity of the reticulo-endothelial system, predisposing to tuberculosis, and on the psychological side the peculiar splitting of the personality that is observed in schizophrenia. I would even go so far as to postulate a deficiency in oxidative processes in the cerebral cortex to explain the intracortical dissociation in catatonic states and hallucinatory experiences.[2]

TABLE 1

Death Rates from Tuberculosis—males *

	WHITE					COLORED			
Age	1884-1907	1908-17	1918-28	1929-30	Age	1884-1907	1908-17	1918-28	1929-30
17	...	17.1	...	...	17	47.5	80.7	32.0	...
22	2.10	5.9	19.0	6.1	22	35.8	17.1	90.0	66.5
27	11.5	10.9	10.1	...	27	18.5	32.8	40.4	25.2
32	13.3	17.8	17.4	...	32	15.9	16.3	36.7	...
37	13.8	21.3	8.6	4.5	37	19.7	26.9	42.5	10.4
42	15.9	12.6	13.4	4.6	42	43.5	18.4	16.0	...
47	14.4	22.2	8.4	10.5	47	12.8	21.8	2.9	...
52	15.0	21.6	17.9	3.0	52	38.5	20.9	19.3	8.6
57	9.8	10.9	10.6	3.8	57	...	20.2	7.0	...
62	9.6	14.2	11.0	10.0	62	11.0	12.6	...	10.4
67	12.0	25.8	12.5	...	67	8.9	49.6	18.0	12.4
72	21.9	36.9	25.6	12.6	72	35.8	37.8	39.8	21.5
77	17.8	42.4	30.4	17.8	77	35.8	62.7	35.5	...
82	28.6	66.1	25.5	60.9	82	...	94.4	...	...
87	35.8	31.4	40.8	...	87	71.6	...	...	...
92	...	...	...	...	92	...	...	...	...
97	...	...	445.0	...	97	...	...	...	...
Total Rates	14.6	18.6	11.4	5.2		27.0	28.2	21.6	6.8

* The rates are per 1,000 male patients.

[2] Freeman, Walter: Deficiency of catalytic iron in the brain in schizophrenia, *Arch. Neurol. and Psychiat.* 24: 300, August 1930.

TABLE 2

Death Rates from Tuberculosis—Females

	WHITE					COLORED			
Age	1884-1907	1908-17	1918-28	1929-30	Age	1884-1907	1908-17	1918-28	1929-30
17	...	...	...	...	17	77.5	244.0	54.6	...
22	31.8	...	16.9	...	22	31.1	69.7	24.8	24.0
27	6.7	18.8	8.3	...	27	33.4	92.5	55.2	...
32	14.6	...	6.5	8.6	32	44.8	51.4	4.4	25.5
37	11.4	...	5.1	7.5	37	11.6	26.4	11.4	...
42	10.9	...	5.1	7.2	42	22.4	25.3	7.4	...
47	22.7	17.9	2.2	...	47	20.6	38.6	11.8	...
52	34.0	6.2	...	13.9	52	7.8	14.3	4.2	...
57	...	15.9	3.0	...	57	16.7	37.6	4.7	12.5
62	10.3	...	5.2	...	62	43.5	48.9	...	22.2
67	15.9	27.9	17.5	...	67	...	44.5	9.1	...
72	29.0	22.2	11.5	...	72	16.7	...	64.8	54.5
77	29.0	32.7	20.9	...	77	116.5	...	48.6	...
82	...	...	...	...	82	...	...	18.7	...
87	...	...	71.0	...	87	212.0	...	...	...
Total Rates	15.0	9.1	6.2	5.5		30.6	40.6	14.1	8.8

The trend of tuberculosis in this hospital is now definitely downward, as is shown in Tables 1 and 2. This is particularly pronounced in the past two years and applies to all groups. It is attributed in part to the recognition of cases before they become candidates for admission to the hospital, in part to the general reduction of tuberculosis mortality through the educational efforts of various organizations combating tuberculosis, in part to the isolation in special buildings in the hospital of the known tuberculous, and in great part to the removal of early cases from the general wards. Contact infection is now seldom recognized. The recognition of the early cases is due in no small measure to the periodic temperature surveys that have been carried out in this hospital since the recognition of the value of this procedure some years ago. Even under the more favorable conditions that prevail at the present time, the death rate from tuberculosis of the population of St. Elizabeth's Hospital is not far from 600 per 100,000 per annum. The greatest saving has been in younger individuals and has contributed in no small manner to the general reduction in the death rate observed in the past few years.

Syphilis

Syphilis has been diagnosed in 986 cases, an incidence of nearly 18 per cent. As most of the cases of tuberculosis involved the lungs, so the great majority of cases of syphilis have been those affecting the brain, but a small number of cases has been included in which there was fairly definite evidence of syphilis affecting the heart and aorta, or the liver. It is quite probable that infection by the treponema is more common than this, but the clinical and pathologic manifestations in such cases were not sufficiently pronounced to permit a diagnosis. Although syphilis is recognized in a certain number of aged individuals, the brunt of the attack would seem to fall upon those in the prime of life. This constitutes the real tragedy of neurosyphilis. As a general rule, people with paresis have made satisfactory adjustments to society until the development of this organic disease of the brain; they have shown no serious character deviations, have often occupied positions of considerable responsibility. One may look forward to the time when patients will be treated for neurosyphilis before they need hospitalization.

The higher incidence of syphilis in males than in females, and in colored rather than white races, is also borne out by our statistics, which are given in Tables 3 and 4.

TABLE 3

Death Rates from Syphilis—Males

	WHITE					COLORED			
Age	1884-1907	1908-17	1918-28	1929-30	Age	1884-1907	1908-17	1918-28	1929-30
17	8.9	...	...	...	17	23.8	...	32.0	...
22	...	...	4.3	...	22	...	...	30.0	...
27	4.6	7.3	11.2	...	27	4.6	20.5	20.1	50.3
32	8.9	15.1	16.5	...	32	18.5	39.0	41.4	31.0
37	15.0	22.1	21.2	9.1	37	27.1	74.0	81.4	31.3
42	27.2	22.0	27.6	4.6	42	34.2	73.5	64.0	28.1
47	30.7	30.6	25.0	15.7	47	25.6	72.5	38.0	33.0
52	20.3	23.4	17.9	...	52	82.5	52.5	46.5	...
57	10.9	9.2	12.8	...	57	26.8	20.2	24.6	41.3
62	13.2	10.3	11.0	20.0	62	16.5	25.2	47.0	10.4
67	5.9	10.1	7.5	7.3	67	...	19.9	24.9	12.4
72	6.0	12.2	14.6	12.6	72	35.8	37.8	30.0	42.9
77	...	38.4	15.1	...	77	...	21.0	11.8	...
82	...	...	8.5	...	82	...	...	34.5	...
87	...	...	40.9	...	87	...	18.9	...	...
Total Rates	15.1	13.9	15.9	5.5		27.0	39.0	41.6	24.5

TABLE 4

Death Rates from Syphilis—Females

	WHITE					COLORED			
Age	1884-1907	1908-17	1918-28	1929-30	Age	1884-1907	1908-17	1918-28	1929-30
17	...	...	29.8	...	17	...	98.0	27.3	...
22	...	...	...	...	22	7.8	52.4	...	...
27	6.8	9.4	...	...	27	16.6	16.9	31.6	24.0
32	...	5.0	9.8	...	32	9.0	12.9	17.8	12.8
37	11.4	...	2.5	7.5	37	7.8	19.8	18.9	22.6
42	18.2	17.4	7.6	7.2	42	4.5	33.6	22.2	10.5
47	3.5	7.2	4.4	7.8	47	6.9	38.6	8.9	23.6
52	4.9	...	2.6	...	52	15.5	14.3	33.5	10.3
57	...	5.3	9.0	10.0	57	...	18.8	14.0	...
62	5.1	...	...	...	62	14.6	...	7.4	...
67	...	...	...	...	67	...	...	9.1	...
72	...	...	...	...	72	...	20.0	16.1	...
77	...	...	...	...	77	...	61.0	32.4	...
Total Rates	5.1	3.8	4.3	4.4		9.7	20.3	16.6	13.2

A great deal of discussion has centered about the relation of arsenical treatment to the incidence of neurosyphilis. Nowhere are complete data available, since patients cannot be kept under observation for sufficiently long periods to ascertain positively the end results. The data of Marburg [3] upon the latent interval between infection and the development of neurosyphilis indicates that the introduction of arsphenamine was followed by a reduction of this latent period from 14 years to 6.

Nonne,[4] also, has indicated that arsenical treatment is associated with an early development of neurosyphilis in certain cases. The early development of neurosyphilis in soldiers and sailors who have been treated intensively, from the time of the initial infection, with arsphenamine and its derivatives has been disheartening to a number of the medical officers with whom I have talked. The scarcity of neurosyphilis in tropical countries and in other places where syphilis is a universal scourge and where arsenical treatment has not yet penetrated, is pointed to as indicating the danger of treating syphilis in this manner. On the other hand the proponents of arsenical therapy insist that the develop-

[3] Marburg, O.: Ueber die Beziehungen der Frühbehandlung der Syphilis zum Ausbruch der spezifischen Nervenkrankheiten, *Wien. m. Wchnschr.* 73: 616, 1923.

[4] Nonne, M.: Die Behandlung der Spät- und Metalues. *88 Vers. dtsch. Naturforsch. u. Aerzte*, Sept. 27, 1924.

ment of neurosyphilis is an indication of inadequate treatment, pointing to the complete eradication of syphilis in patients who submitted to treatment sufficiently early, and even reporting second and third primary lesions. The rarity of neurosyphilis in patients presenting marked cutaneous lesions, and the rarity of rashes in our paretics, seem to point to the probability, which has often been expressed, that a natural immunity to syphilis is built up in the body, at least in part by the reaction in the skin and that this prevents the development of more serious visceral lesions. Arsphenamine, by killing off the greater number of parasites, prevents the development of the natural immunity and yet predisposes to the lodgment of treponemata in the viscera. At the present time there seems to be a tendency to treat primary syphilis intensively and latent syphilis conservatively. The arsenical treatment of the secondary stage is marked by such gratifying temporary results, and the danger of contagion from florid syphilis is so great that the time has not yet come when the physician may treat such patients in a conservative manner. Nevertheless from the standpoint of end results, it would seem that a vigorous cutaneous response to luetic infection was the most reliable guarantee against subsequent visceral syphilis that has yet been found. Nor is the problem related to neurosyphilis alone. The recent paper by Gager [5] upon late cardiovascular syphilis is a case in point.

The most hopeful prognostic indication from the standpoint of neurosyphilis is contained in the study by Moore and Hopkins,[6] who showed that future neurosyphilis can be fairly accurately forecast or almost absolutely excluded by examination of the spinal fluid.

It seems generally agreed, now, that arsphenamine and its derivatives are of little use in combatting frank or even latent neurosyphilis. The more recent introduction of tryparsamide has been followed by favorable reports from a number of clinics where patients could be kept under constant observation and treatment, but in this hospital the results have not been very striking, and relapses have occurred in patients who have had more than 50 injections.

My own data would seem to have some bearing upon the treatment of syphilis in both phases, before and after the development of neural

[5] Gager, L. T.: Syphilitic heart disease. A consideration of early diagnosis and factors in immunity. *Am. J. Syph.* 13: 111 (July) 1929.

[6] Moore, J. E., and Hopkins, H. H.: Asymptomatic neurosyphilis, *J. A. M. A.* 95: 1637 (Nov. 29) 1930.

involvement. I have plotted the death rates of white males from syphilis for three epochs in Chart 1. The figures in the columns of Tables 3 and 4 may also be consulted. The era from 1884 to 1907 may be considered the pre-salvarsan era when mercury and iodides did little to delay the course of paresis. The period from 1908 to 1928 followed the introduction of arsphenamine and overlapped somewhat that of malarial treatment. The past two years, at least in the case of the white males, have shown the effects of malaria therapy. It is noted that the curves for 1884-1907 and 1908-1928 are closely parallel except for the earlier decades. The death rate at 25-29 has doubled in this period and at 30-34 has almost doubled. The death rate as a whole has shown little change. This would indicate an earlier onset of paresis since the introduction of arsphenamine, but not necessarily that the incidence of paresis had been increased. In the case of colored males, however, there is a marked increase in the death rates from paresis, as well as a preponderance in earlier years. The same is true for colored females, whereas white females, if anything, have shown a slight general reduction.

The problem does not rest upon this last group, however, because of the small percentage of cases. The shift to earlier decades in the male coupled with greatly increased incidence of paresis in the colored race following the introduction of arsphenamine might indicate that arsenical therapy had been the cause. On the other hand, antiluetic treatment of these individuals is a factor that has been impossible to control. The records coming with the patient often say nothing in regard to previous arsenical therapy, and the majority of patients are in such poor mental condition that they cannot recall either infection or treatment of it. Many patients are demonstrated to be syphilitic only after admission to the institution, although this is not as common as formerly. Again, the age at which syphilitic infection first occurred is not known in most instances, so that the latent period cannot be estimated.

In the face of these unknowable factors it is difficult to draw incontrovertible conclusions from the data at hand. Nevertheless, it is my own impression that the introduction of arsphenamine has increased the incidence of neurosyphilis as well as its precocity. Numerous cases might be cited in which patients were under constant observation and intensive treatment from the period of their initial infection until their death from full-blown paresis, all within a space of three years. Whereas arsphenamine is likely to abort syphilis in its earliest stages,

its curative value, when the infection is disseminated, is doubtful. In preventing the development of natural immunity, arsphenamine predisposes to precocious neurosyphilis. Its tonic value in frank parenchymatous neurosyphilis is considerable, but its curative value is nil.

A more pleasing picture for white males is presented by the curve (smoothed) for the past two years. Moreover the total death rate from syphilis is but little above that of the white females. This would seem to be due almost entirely to the successful use of malaria in the treatment of neurosyphilis.

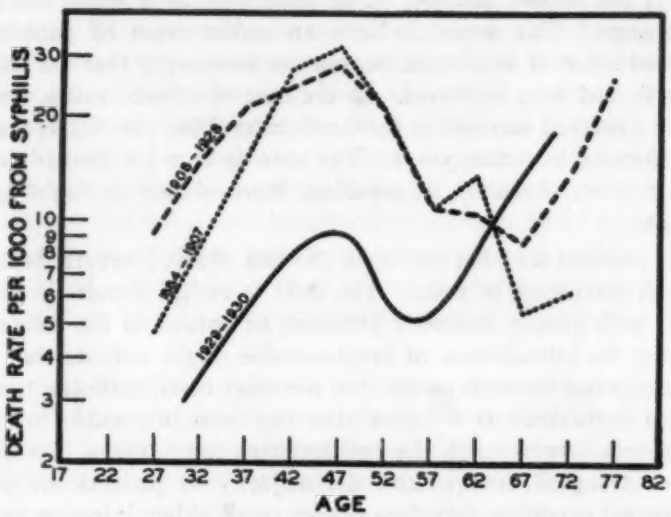

FIG. 1. DEATH RATE PER 1,000 FROM SYPHILIS—WHITE MALES (1929-1930 smoothed mean)

Many articles have come from this and other hospitals dealing with the beneficial effect of fever therapy upon paresis. In the years immediately following the introduction of malaria therapy, the death rate from paresis seemed to be considerably increased, probably due to the poor condition of the patients receiving it. Only within the past three years has the full effect of this treatment been in evidence. The death rate from syphilis of all forms has been reduced 65 per cent, the greatest saving, of course, being in paretics. The older individuals in the 1929-1930 column for white males represent mostly cardiovascular and hepatic forms of lues.

During this same period the colored males have shown a significant reduction, but not nearly so marked as in the case of the white males. The colored females show less reduction in the death rate during the past two years. The reason for this discrepancy between white and colored is undoubtedly the fact that the negro is highly resistent to inoculation by malaria, and seldom has a satisfactory set of paroxysms. This being the case, Eldridge is now treating such patients by means of diathermy.

The outlook for patients with neurosyphilis has been altered greatly by the introduction of malaria. Of the patients admitted to St. Elizabeth's Hospital with paresis, some 30 per cent have the disease arrested but are mentally too dilapidated to make a satisfactory adjustment outside the hospital. There is a tendency, however, to administer malaria to patients with early and even latent neurosyphilis before their hospitalization becomes necessary. This tendency is a good one and should be fostered, since the recovery rate can be more than doubled by early treatment. In my own experience, two out of three patients return to their former occupation in full economic status.[1] The risks of the treatment are less on account of the relatively good physical condition of the patients.

Cancer

Malignant disease has been found as a significant cause of death in 302 necropsies, representing an incidence of 5.4 per cent. Most of the tissues of the body have been affected. In only a small percentage of cases has a growth in the brain been looked upon as the exciting cause of the psychosis, and the cerebral neoplasms appear to be no more frequent in the psychotic population than in the general population. The actual incidence of carcinoma is higher than that given above, since the microscopic growths found in the hypophysis, prostate, thyroid, etc., have been excluded from the tabulations in this study, although in studying psychologic correlations, all malignant growths have been included. The age factor is of great importance in carcinoma, the curve of the death rate rising quite steadily with increasing age to reach its peak in the ninth decade. Even our own limited statistics are clear cut in this regard. They are not so clear, owing to small figures, for com-

[1] Freeman, Walter: Therapeutic malaria in private practice. *Southern M. J.* 24:933 (Nov.) 1931.

paring the death rate from cancer of the hospital population with that of the general population, but the proportion would seem to be of the order of three to one. Lest this be taken as indicating a very extensive malignancy among the psychotic population, it may be said that in something like 30 per cent of the cancer cases coming to necropsy, the clinical diagnosis of cancer has not been made. If this is true with the facilities of a well equipped hospital constantly at command, it follows that the data appearing on the death certificates of the general population are far from revealing the true incidence of malignant disease. It is quite within the realms of possibility that the increased incidence of cancer reported in vital statistics is wholly attributable to increased accuracy in clinical diagnosis and to increased longevity. On the other hand, certain types of malignant disease, such as primary carcinoma of the lung, are undoubtedly being found more frequently than they were in former years.

TABLE 5

Death Rates from Malignant Tumors—Males

	WHITE					COLORED			
Age	1884-1907	1908-17	1918-28	1929-30	Age	1884-1907	1908-17	1918-28	1929-30
22	...	...	...	...	22	...	...	...	...
27	1.54	0.90	1.02	...	27	...	4.08	...	...
32	...	0.90	1.71	...	32	...	...	...	...
37	...	0.90	0.79	...	37	...	3.42	...	...
42	1.33	1.05	1.70	...	42	6.24	...	3.22	...
47	4.81	1.42	2.68	2.58	47	2.58	...	5.76	...
52	1.91	1.77	3.33	3.04	52	...	10.50	3.90	25.81
57	4.87	1.81	11.49	3.75	57	4.54	6.68	3.48	...
62	7.30	9.06	5.38	9.97	62	5.49	25.64	5.16	10.41
67	9.58	10.11	17.62	14.56	67	8.88	9.88	12.05	12.38
72	8.95	13.48	25.58	62.88	72	...	...	10.02	21.53
77	17.90	27.08	40.42	71.29	77	35.77	41.88	...	...
82	21.50	37.76	50.88	30.38	82	...	...	34.48	...
87	35.82	...	122.42	547.50	87	...	188.44	74.72	145.56
Total Rates	5.03	4.20	5.45	6.10		2.78	4.81	4.32	6.84

The data from this hospital are disappointingly inadequate as far as revealing any trend in the incidence of malignant disease. The cases are too few, and their occurrence in the extreme of life is associated with too marked variations to be of any true value. The figures are

TABLE 6

Death Rates from Malignant Tumors—Females

	WHITE					COLORED			
Age	1884-1907	1908-17	1918-28	1929-30	Age	1884-1907	1908-17	1918-28	1929-30
22	...	...	...	...	22	...	17.38	...	...
27	...	...	...	...	27	...	...	...	...
32	...	...	3.28	...	32	...	6.44	4.44	...
37	...	...	...	...	37	3.88	...	7.59	...
42	...	...	2.53	...	42	4.54	8.38	...	...
47	3.31	...	...	...	47	13.65	12.75	3.03	...
52	...	...	2.56	...	52	17.47	11.40	12.55	...
57	...	5.28	12.78	...	57	...	...	4.70	...
62	...	22.92	15.74	21.36	62	...	...	14.76	...
67	5.28	14.04	23.28	27.33	67	...	...	9.14	...
72	...	...	11.52	...	72	6.73	...	...	...
77	...	...	20.88	60.00	77	...	61.14	...	40.00
82	...	...	16.90	...	82	...	...	18.74	...
87	...	...	142.16	...	87	116.25	...	178.04	...
Total Rates	.79	2.88	6.22	4.44		6.47	5.09	6.01	1.46

given in Tables 5 and 6. As far as concerns the white males who constitute 60 per cent of the hospital population, there is little variation in the gross rates over a period of almost 50 years. Nevertheless, in the decade 70 to 79, each successive period shows a substantial gain over the preceding. These figures are taken from large numbers of individuals in whom the expected incidence of malignant disease is high.

Conclusions drawn from the females are at absolute odds and are not to be trusted. In the whites the incidence has increased fivefold, whereas in the colored it has decreased in the same proportion, and in the same time. Only at age 67, where comparative figures are available, is there shown an increase in cancer among white females parallel with that of white males. It will be recalled from the previous study that the period 1908-1917 was associated with the highest average age of white males in the hospital, and at the same time the gross death rate from cancer is the lowest. Nevertheless, this period was also marked by the poorest pathologic reports, and many instances of carcinoma in out of the way places may have escaped recognition.

The most that can be said of the cancer situation as expressed in these statistics, based upon the autopsied population of the hospital, is that cancer is showing no alarming increase, and that it has probably

always been responsible for a death rate of about 500 per 100,000 in the hospital population. Compared with the recent developments in the general mortality trends for tuberculosis and syphilis, these differences sink into insignificance. In aged individuals, malignant disease is a very important cause of death, but it should be emphasized that the death rate for malignant disease climbs significantly above that from tuberculosis only around the age of 80.

It was hoped that the figures obtained from this study on malignant disease in this hospital would permit of some deductions concerning the situation in the country in general. The findings, however, are so contradictory that no certain conclusions can be drawn. Nevertheless, in view of the absence of any great increase in the incidence of malignant disease in the period covering the past 45 years in this hospital, the opinion might justifiably be advanced that the apparent increase in the significance of carcinoma in vital statistics is due to the greater longevity of the people and greater diagnostic acumen of the physicians rather than to some inherent biologic change that makes for the development of malignancy.

The only survey of the cancer situation in psychiatric institutions of this country with which I am familiar is by Hoffman.[8] This is based of course, upon the death certificates, probably reinforced to some degree by necropsy records. Because of the large error in clinical diagnosis in psychotic individuals, I cannot accept the results as indicating a pronounced increase in the death rate from cancer. My own data, calculated from the number of cancer cases found at necropsy, the necropsy percentage being well over 50 per cent, are believed to be a better guide to the situation. Hoffman gives the following rates for the New York State Hospitals:

	1916-1920	1921-1925	1926
Male	102.27	152.25	156.27
Female	220.16	232.80	277.92
Total	165.75	195.13	222.14

It is scarcely thinkable that the incidence of carcinoma has increased 25 per cent in the past ten years as would be indicated by the figures presented. My most reliable figures indicate an increase of 8 per cent

[8] Hoffman, F. L.: San Francisco Cancer Survey; fifth preliminary report, Newark 1929, Prudential Insurance Co. of America. Page 9.

in 45 years, which is associated with an increase in the average age of the hospital population. This figure is too low to be out of range of the probability of error on the basis of random selection.

In summing up it may be stated that there is insufficient evidence to draw valid conclusions with regard to trends in mortality from malignant disease, but that there is no indication of a serious rise.

Part 2.

THE ASSOCIATION OF TUBERCULOSIS, SYPHILIS AND CANCER

In this second part of the paper I wish to present further data upon the three diseases already studied, but this time from a somewhat different angle. The simultaneous occurrence of two diseases is no novelty, but in view of the interest recently aroused by Pearl and others in the supposed mutual antagonism between tuberculosis and cancer, it was considered that the material at hand was suitable for study. The complex statistical study of Pearl [9] indicated a definite antagonism between the two disorders; so much so that under his direction some attempts at tuberculin therapy of malignant disease were instituted, and favorable results were reported. On the other hand, and with a large statistical material, Carlson and Bell [10] reached the conclusion that the only way active tuberculosis could prevent carcinoma was by killing the patient earlier. Moreover there seemed to be greater antagonism between active tuberculosis and heart disease than between active tuberculosis and cancer; and the same thing held for heart disease and cancer.

A number of papers have appeared, taking sides upon a question that is almost as old as histopathology. Pearl's method, however, seems to me to yield the most reliable results, but our trials of tuberculin therapy in cases of cancer have met with little success. The explanation of the supposed antagonism, I believe, lies not so much in the provocative agent as in the fundamental biologic trend of the individual. In No. III of this series I pointed out the massive preponderance of tuberculosis in the schizoid group, and the low incidence of malignancy; also the high incidence of malignant disease in the cycloid and paranoid types, types that are resistant to tuberculosis. I discussed this ques-

[9] Pearl, R.: Cancer and tuberculosis, *Am. J. Hyg.* 9:97, 1929.

[10] Carlson, H. A., and Bell, E. T.: A statistical study of the occurrence of cancer and tuberculosis in 11,195 post mortem examinations. *J. Can. Res.* 13:126, 1930.

tion further in No. V of this series. Furthermore, although the death rate from tuberculosis rises slightly with increasing age, that for malignant disease rises from a few parts per million in the twenties, to more than 1,000 per 100,000 in individuals over 75. Consequently a direct

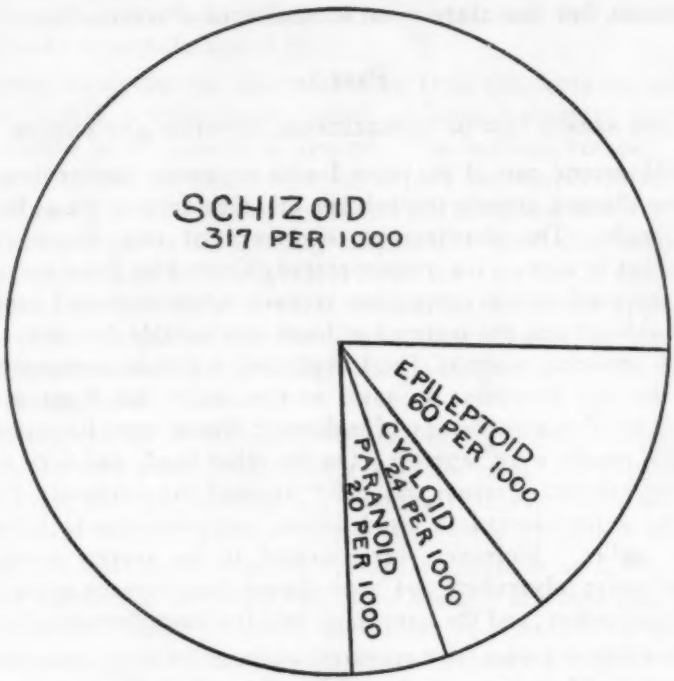

FIG. 2. COMPARATIVE INCIDENCE OF ACTIVE TUBERCULOSIS

comparison is not altogether suitable. Pearl has overcome this objection by a case-for-case selection that is admirable. Nevertheless, the material available at the Johns Hopkins Hospital is open to criticism in that it represents highly selected material, especially since the treatment of cancer is so greatly emphasized. Carlson and Bell's material is better adapted to a study in this respect. My own material is believed to represent a satisfactory selection as far as the psychotic population is concerned, but the high incidence of tuberculosis in young individuals, and the high incidence of carcinoma in old individuals must be kept in mind.

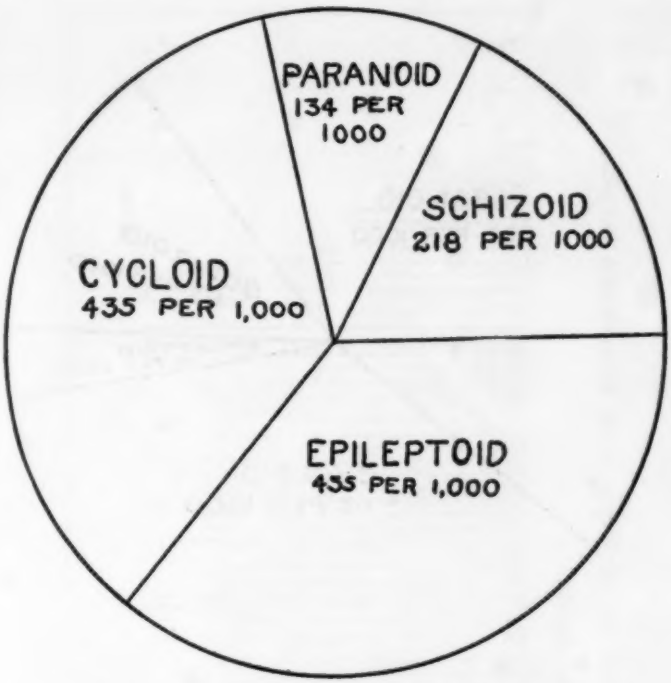

FIG. 3. COMPARATIVE INCIDENCE OF SYPHILIS

In order to balance and contrast the possible antagonism between tuberculosis and cancer, I have chosen syphilis, and treated it in the same way. The age distribution of syphilitics is more nearly that of the tuberculous, and is somewhat farther removed from the cancerous than is that of the tuberculous. Viewed from the psychological aspect, syphilis is most apt to be found in the cycloid and epileptoid, and rather infrequently occurs in the paranoid. On the other hand, the epileptoid individual rarely has cancer. In Charts 2, 3 and 4, I reproduce from my earlier paper the pie diagrams illustrating the comparative incidence of tuberculosis, syphilis and malignant disease in order to bring out more graphically the differences. These differences seem to rest upon a fundamental biologic character that is expressed in bodily conformation, chemical, metabolic and immunologic properties, and psychological peculiarities, and I would invoke these principles rather than any peculiarity of the exciting cause in explaining the supposed mutual antagonism of various diseases.

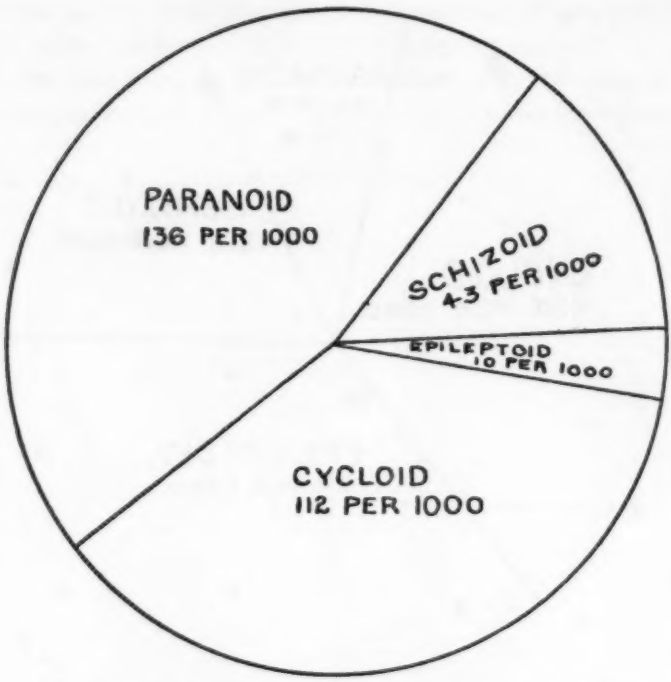

FIG. 4. COMPARATIVE INCIDENCE OF CARCINOMA AND OTHER MALIGNANT TUMORS

In Chart 5 is indicated the occurrence of syphilis, tuberculosis and cancer in 1122 males examined between 1918 and 1927.

The results obtained from this study of supposed mutual antagonism are presented in Table 7. The calculations are simply those for determining what would be the concurrence of two diseases, were chance the only governing factor. Over against this calculated figure is set the actual number of cases encountered, and in the last column is indicated the percentage of actual results compared with the calculated.

With the qualifications previously expressed, therefore, concerning differences in age and in behavioristic peculiarities, it may be said that there is an unlikelihood of either tuberculosis or syphilis occurring in the same individual in association with malignant disease, but I cannot accept Carlson and Bell's explanation that the only reason a patient does not die of cancer is that tuberculosis kills him first. The rising death rate from tuberculosis with increasing age is evidence in point.

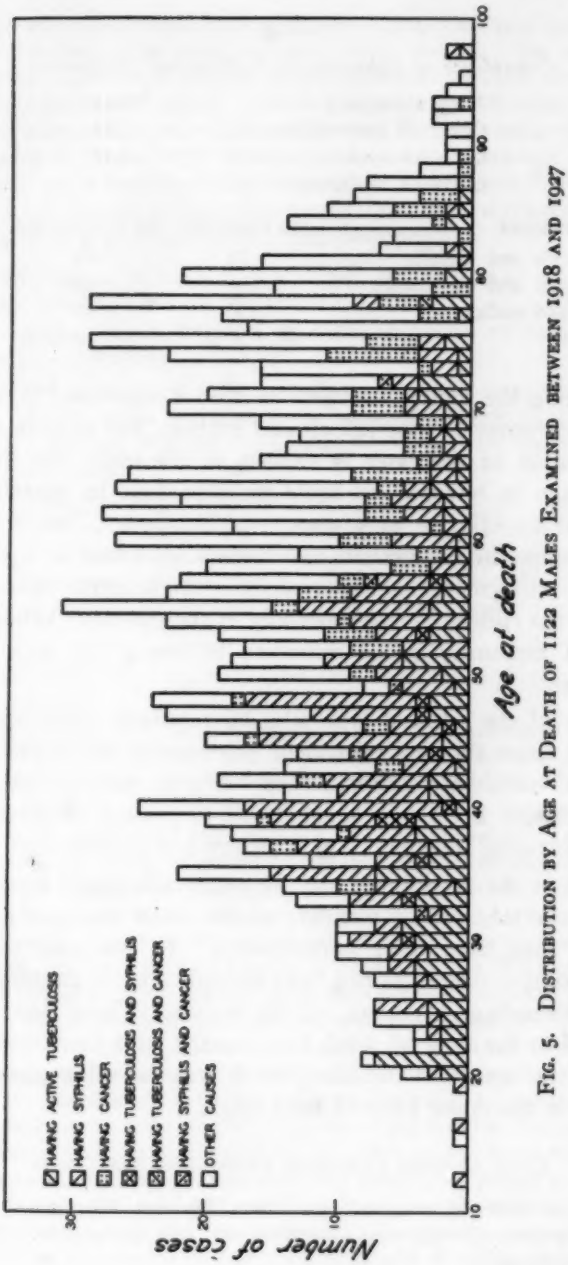

FIG. 5. DISTRIBUTION BY AGE AT DEATH OF 1122 MALES EXAMINED BETWEEN 1918 AND 1927

TABLE 7

Coincidence of Tuberculosis, Syphilis and Malignancy

Total necropsies	5223
Cases of tuberculosis	922
Cases of syphilis	927
Cases of malignancy	285

DISEASES	NO. OBSERVED	NO. CALCULATED	PERCENTAGE
Tuberculosis and syphilis	73	164	45
Tuberculosis and malignancy	9	51	18
Syphilis and malignancy	8	52	15
All three	0	9	0

Concerning the mutual antagonism that is expressed in the figures for the concurrence of tuberculosis and syphilis, less is to be said. The age distribution of the cases is similar, in the main, but here, again, the difference in biologic substrate as expressed in personality type would seem to offer a satisfactory explanation. The incidence of syphilis is great in the cycloid and epileptoid, while it is low in the schizoid. The incidence of tuberculosis is high in the schizoid, while it is low in the epileptoid and especially in the cycloid. Whether it is a question of immunity or of exposure is treated in paper No. III of this series.

Patients of the schizoid type who have paresis make up the great majority of those showing coincident tuberculosis and syphilis. These cases are of sufficiently common observation to need no special detailing. However, it is thought worth while to present abstracts of cases showing coincident tuberculosis and cancer and syphilis and cancer.

In some of the cases there may be reasonable doubt concerning the activity of the tuberculous process; on the other hand, other cases of coincidence may have come unrecognized. In one case recently observed, tuberculin treatment had been administered in an effort to check a rapidly spreading carcinoma of the cervix. There was little local change, and at the time of death two months later there was found in addition to the cancerous condition, fresh areas of tuberculous bronchopneumonia in the upper lobes of both lungs.

Cases Showing Coincident Tuberculosis and Cancer

CASE 1. White male, 66 years of age. (St. Eliz. Aut. No. 279, Oct. 1, 1888). Diagnosis: Chronic mania, duration one year ten months. (Pathologist, Blackburn.)

The body was considerably emaciated. The costal cartilages were deformed by previous fracture. The brain showed nothing unusual. The heart showed brown atrophy. There were "strong pleuritic adhesions on both sides; extensive tubercular deposits in both apices." "Stomach. At the pyloric orifice was a large flat cancerous mass, and in the vicinity of this end of the stomach was a mass of enlarged lymphatic glands which presented a cancerous appearance. The tumor was rather soft and on gentle pressure a whitish substance exuded from points on its surface. Liver and gall bladder were normal. No secondary cancerous deposits in any organ except the lymph glands above mentioned."

CASE 2. White female, 66 years of age. (Aut. No. 1758, May 12, 1903, pathologist, Blackburn.)
Diagnosis: chronic dementia, 15 years, carcinoma of right breast, pulmonary tuberculosis.

History. "Patient has been ill for several months, hectic fever, cough with expectoration. Sputum exam. revealed the presence of tubercle bacilli. The left lung was more involved than the right. Dr. Kerr diagnosed the tumor of the right breast as carcinoma."

Necropsy findings. "Body emaciated. Scar of right breast supposed to be cancerous." The brain showed senile atrophy, the heart and aorta were normal. "The right lung weighed 500 grams; left too adherent to remove. Both were full of tubercular cavities and caseous masses. No adhesions on right side; left side when not adherent filled with sero-pus." The other organs showed nothing except for gall-stones.

CASE 3. White male, 76 years old. (Aut. No. 1944, Feb. 23, 1905, pathologist, Blackburn.)
Clinical diagnosis: Chronic dementia—sunstroke—pulmonary tuberculosis.

Patient was admitted in 1888. The notes in 1902 show that there was pain in the left side with evening rise of temperature, and on two occasions chills with vomiting. In September, 1904 tubercle bacilli were found in the sputum. There was steady loss of weight with much pain and limitation of motion in the shoulder.

Necropsy findings. Body of an old man greatly emaciated; abdomen scaphoid. "Lungs were not removed on account of firm adhesions to the chest wall over entire surface—incised in situ. Substance is pale, somewhat carbonotic. At left apex is large cavity, size of an orange, ragged, filled with purulent matter. Right apex, few small cavities. Pleural adhesion is apparently not of tubercular origin —no disease of lungs except at apices. Liver 1090 grams. Adhesions of omentum, hepatic flexure of colon and of duodenum found to mass in region of gall bladder. On removal of liver a gall stone was noted in the apex of bladder above a rounded hard mass occupying position of gall bladder. The liver tissue in the neighborhood showed fibrosis. Incision of the bladder and mass from above downwards revealed round hard new growth occupying greater portion of gall bladder. A cylindrical stone large as a pecan nut with a facet below lay in a little pocket above the tumor and a stone of similar size lay in a pocket below the tumor, just at exit of cystic duct. Both stones were bathed in thick mucoid material and the bladder walls were inflamed and covered with the same material. Ducts were normal. The cut surface of the tumor showed a hard uniform mass with a few small cystic cavities

and some gelatinoid material on scraping, apparently a cystadenoma of the cylindrical cell variety. Below the tumor showed a smooth knuckle-like projection into the remains of the gall bladder. No metastases were found in liver or glands of lesser omentum. Tumor is apparently primary in gall bladder and apparently separated the calculi, and in growing downward was more or less encapsulated by the bladder walls and liver tissue. The serous surface of the small intestine showed dark discoloration in spots with miliary tubercles and enlarged lymph vessels running into mesentery. Mesenteric glands enlarged. Mucus surface showed eight circular ulcers with red and swollen edges situated at some distance from the cecum. The base in one case was the thickened serous coat."

CASE 4. Colored female, 38 years old. (Aut. No. 1984. June 14, 1905, pathologist, Blackburn.)
Clinical diagnosis: Acute mania: carcinoma of cervix uteri.

History. Patient was admitted in 1902 in an acutely manic state. She quieted down, however, and worked regularly in the sewing room. She lost weight and suffered from cystitis. On Feb. 7, 1905, she ceased work on account of the effects of a carcinoma of the cervix. After being in bed a month she had an attack of bronchopneumonia which never cleared up completely and she continued to have slight irregular fever. She suffered severe pain and became greatly emaciated.

Necropsy findings. Emaciated light colored female. "Lungs and pleura. Right pleura adherent over the middle and lower lobes. Left adherent to diaphragm. Left lung 520 gm. The lower two-thirds is completely consolidated with tuberculosis: one small cavity. A few small nodules in the upper lobe (T. B. bronchopneumonia). Right lung 400 gm. Dense fibrous adhesions between lower and middle and upper lobes. Lung consolidated with pneumonia but no definite tubercles found. Uterus: there is an extensive ulcerative carcinoma of the cervix uteri. The upper part of the vagina is involved and the ulceration extends to the peritoneum over an area about 2 cm., in diameter just behind the bladder. There are numerous enlarged lymphatic glands in the region of the uterus and extending nearly up to the diaphragm. The largest is about the size of a walnut and is broken down in its center. There are several small polypi in the uterine cavity and three intramural fibroids of the uterus, the largest being about the size of a walnut." The diagnosis of epithelioma was confirmed by microscopic examination.

CASE 5. White male, 73 years old. (Aut. No. 2963, Jan. 23, 1912. Pathologist, Pfeiffer.)

History. Patient was admitted in June, 1911 with a poor memory and delusions of persecution. Arteriosclerosis was outstanding. The heart was irregular. There was bilateral ptosis and tremor of the extended fingers. On the day of his death he complained of feeling sick, had slight fever but undressed and got into bed. Fifteen minutes later he was found dead.

Necropsy. Some small areas of hemorrhagic pachymeningitis were found and a small soft irregular growth involving the bone in the left posterior fossa. This may have been related to a small hypernephroma (proven microscopically) which was found in the left kidney. "In the apices of both lungs are active areas of tuberculosis." Death was due to atrophic cirrhosis of the liver with acute purulent peritonitis, and acute degeneration of the myocardium.

CASE 6. White female, aged 67 years. (Aut. No. 2995, March 21, 1912. Pathologist, Lafora.)
Clinical Diagnosis: Dementia precox; carcinoma of breast.

History. Patient was admitted to the institution in 1885. Of recent years she had become greatly demented with hallucinations. She was noisy at times. Her physical health was good until March 18, 1912 when she complained of abdominal pain. A growth was found in the right breast with enlarged glands in the axilla. There was tenderness over the whole abdomen.

Necropsy. A cystic tumor occupied the location of the hypophysis and compressed the chiasm destroying the inner portions of the optic nerves. The cyst contained clear fluid. The sella turcica was enlarged. "A number of tubercles were found in the superior and inferior lobes of the left lung. Right lung inflamed. Pleuritic adhesions." There was purulent peritonitis of uncertain origin. (Possibly tuberculous?) This report is rather brief and does not mention the mammary tumor.

CASE 7. White female, 64 years old. (Aut. No. 5166, Nov. 19, 1927, pathologist, Freeman.)
Clinical diagnosis: Mental deficiency without psychosis; pulmonary tuberculosis.

History. Patient was admitted in 1899. She was hypochondriacal, complaining, yet she made a good hospital adjustment. Her first indication of tuberculosis was in 1911 when she lost weight, had fever, and upon X-Ray examination showed apical consolidation. No tubercle bacilli were ever found and she continued in fair health for fifteen years finally developing an acute pneumonic condition, supposed to be tuberculous, from which she died in nine days.

Necropsy. There were ancient fibrotic and caseous masses at both apices with a few fresh daughter tubercles that showed strict limitation by fibrous encapsulation. In other parts of the lung there were simple bronchopneumonic areas. In addition there was acute hemorrhagic pyelitis and suppurative nephritis, nontuberculous in type. The thyroid gland was slightly enlarged and irregular, showing a firm "whitish nodule with extensions out into the thyroid tissue." This proved, on microscopic examination, to be a small scirrhous carcinoma.

CASE 8. White male, 78 years old. (Aut. No. 5288, Oct. 16, 1928, pathologist, Freeman.)
Clinical diagnosis: Senile psychosis; arteriosclerosis.

History. Patient was admitted in 1925, showing irritability, disorientation, loss of memory. He had advanced arteriosclerosis, and during his residence suffered a number of "weak spells" probably cardiac in origin. Intermittent claudication developed and finally thrombotic gangrene of the left leg. On account of his poor physical condition operation was not performed and he died of pneumonia 8 days later.

Necropsy. There was thrombosis of the femoral artery with gangrene of the left leg, confluent bronchopneumonia. In addition a scirrhous carcinoma was found in the stomach with metastasis to the regional lymph nodes and liver. "The liver is slightly reduced in size, with a thin capsule, showing half a dozen small whitish nodules. More of these nodules are seen on section, mostly belonging

to the type expected in metastatic lesions, but one of them resembles the lesion in the spleen, consisting of a calcareous shell enclosing caseous material, and measuring about 5 mm. in diameter. It is suspected that both these and the splenic lesions are tuberculous. The largest of the metastatic lesions is about 1 mm. in diameter." Microscopically the carcinomatous nature of the gastric lesion was recognized. In the same section of liver submitted to microscopic examination there was a hyalinized tubercle, a metastatic carcinomatous nodule, and a relatively fresh tubercle with cellular reaction and giant cell formation that left no doubt as to its nature. In the pulmonary apices were scars of ancient tuberculosis, but no signs of activity.

CASE 9. White female, 66 years old. (Aut. No. 2153, Nov. 2, 1906, pathologist, Blackburn.)

Clinical diagnosis: Senile dementia; epithelioma of forehead.

History. Patient was admitted June 15, 1906 in a state of extreme dementia. An epithelioma of the forehead was noted at that time.

On August 27 she became comatose with right-sided hemiplegia and aphasia. She recovered somewhat but finally died of intercurrent bronchopneumonia.

Necropsy. "The right eyebrow is the seat of a growth covered with crust, with scar tissue in the central portion and in the periphery. This is an epithelioma. There is a small flat growth situated at the left nostril. The pleura of the left lung shows numerous small white nodules, probably miliary tubercles. The pleura of the right shows a very few scattered nodules of a similar character. There is a limited scar, probably tubercular, at the apex of the left lung. A section of the left lung does not reveal any miliary tubercles in the lung substance.

A section of the right lung shows some obsolete tubercular scars in the apex. No recent tubercles in the right lung."

Coincident Syphilis and Carcinoma

Syphilis and carcinoma are found in association in the same subject even less frequently than are tuberculosis and carcinoma. This mutual antagonism has been recognized by pathologists and various explanations have been adduced.

The treponema may be particularly injurious to the neoplastic cells. The sclerosing effect of syphilis may prevent the development of a carcinoma. On the other hand, a chronic inflammatory process, such as that leading to luetic ulceration of the stomach, would seem in theory to favor the development of a neoplasm. Such, however, is rarely the case. Moreover, gummata seldom become cancerous. The endarteritis aroused by syphilis may be unfavorable to the development of cancer.

Similar arguments might just as well be advanced to explain the antagonism between cancer and tuberculosis. Turning to the biologic reaction type, however, it appears that syphilis is preponderant in the

epileptoid and at its lowest in the paranoid, while carcinoma shows exactly the reverse distribution. Furthermore the age of the luetic is distinctly below that of the cancerous: none of our cases of concurrent cancer and syphilis was over 60 years old, whereas only one of our cases of concurrent tuberculosis and cancer was under this age. The records of the eight cases of concurrent syphilis and malignant disease are given below.

A further case, at first supposed to be a concurrence, is now believed to have been a syphilitic ulcer of the stomach in association with tabes.

Cases Showing Coincident Syphilis and Cancer

CASE 1. White male, 50 years old. (Aut. No. 929, Feb. 25, 1895, pathologist, Blackburn.)
Clinical diagnosis: General paralysis of the insane, 2 years 10 months' duration.

Necropsy. The brain was atrophic with thickening of the meninges, gaping of the fissures, shrinkage of the optic nerves and puckering of the frontal convolutions when the pia mater was stripped. "A large hard moderately lobular mass lay beneath the stomach somewhat to the left of the median line. On examination of the tumor mass it was found to be composed of caseous glands. The greater portion of the duodenum was adherent to the mass but there was no growth in the duodenum so far as could be distinguished. Dissection revealed no connection with the tumor mass except by chronic adhesions similar to those elsewhere. The alimentary canal was thoroughly searched without finding any ulcerations or other lesions. The stomach showed chronic catarrh and apparent atrophy; the pancreas was not involved though it was in close proximity to the growth. Some of the other mesenteric glands in the vicinity were also involved, i. e. enlarged and caseous . . . . Liver, weight 1590 gms. The capsule was firmly adherent to the diaphragm and to free it was impossible. The tissue was found to be studded with small yellowish white masses; these nodules were firm and hard and projected slightly above the cut surface on section. They were supposed to be secondary."

Although no microscopic examination was performed the absence of tuberculous lesions elsewhere led to a diagnosis of lymphoblastoma of the retroperitoneal nodes with metastatis to the liver.

CASE 2. Colored female, aged 41 years. (Aut. No. 1175, May 26, 1897, pathologist, Blackburn.)
Clinical diagnosis: General paralysis of the insane, duration 18 months.

Necropsy. Brain. "Pia mater edematous, vessels moderately full of blood; membrane easily removed from cortex; arteries at base free from disease. Convolutions shrunken over the whole brain. Brain substance edematous, of moderate consistence, perivascular spaces wide. Ventricles dilated, lining smooth. No gross lesions in the cerebellum, pons and medulla." There was hypostatic pneumonia and degeneration of the cardiac musculature. "Uterus. The neck of the uterus showed a ragged irregular lesion with apparent loss of substance involving

the whole of the external portion of the neck of the uterus. On incision it was seen that the ulceration had penetrated deeply. The tissue of the neck seemed to be considerably blackened and deeply penetrated by the ulcerative process. Where the organ came into juxtaposition with the bladder the ulcerative process had extended through as far as the mucous membrane of the bladder but the latter did not appear to be penetrated. . . . . No tumor deposits were found in any other of the organs of the body. . . . . The condition of the uterus was not very clear but it was supposed to be an epithelioma."

CASE 3. White male, 60 years old. (Aut. No. 3026, June 21, 1912, pathologist, Lafora.)
Clinical diagnosis: Paresis; Cardiorenal disease.

History. Patient had a chancre 30 years before, with treatment. On admission he was oriented and had no delusions or hallucinations but was apathetic and had no insight. There was dysarthria, anisocoria, ataxia and irregular reflexes. During the last month of his life he became very weak, had vomiting spells and pigmentation of the addisonian type.

The serological findings were consistently negative.

Necropsy. The brain was shrunken with numerous small infarcts and hemorrhages scattered through both the hemispheres and the cerebellum. The vessels were sclerotic, but neither grossly nor microscopically were the changes those of paresis. Histologically the vascular lesions in the brain were considered to be luetic. A tumor 12 mm. in diameter was found in the cortex of the left suprarenal capsule, which upon microscopic examination proved to be a hypernephroma.

CASE 4. White male, age unknown. (Aut. No. 4075. Nov. 25, 1918, pathologist, Lewis.)
Clinical diagnosis: Dementia paralytica.

History. Patient was admitted three months before death in a wildly excited state. He had many conflicts, was noisy and destructive. It was impossible to obtain a mental examination. The serology was typical of paresis. He died of intercurrent bronchopneumonia.

Necropsy disclosed a large brain weighing 1535 grams, with slightly thickened pia exhibiting numerous tortuous small vessels. The mesial surfaces were adherent, the convolutions were smooth and flattened, showing no atrophy. Along the lesser curvature of the stomach was an ulcerated growth which had penetrated into the liver forming a secondary abscess. It was believed to be carcinomatous, although no microscopic sections were made.

CASE 5. Colored female, 60 years old. (Aut. No. 4253, May 18, 1920, pathologist, Lewis.)
Clinical diagnosis: Paresis; unresolved pneumonia.

History. The patient was untidy, bedridden and emaciated on admission a few weeks before death. Coördination was poor, the pupils were rigid, there was dullness on the right side with dyspnea and a foul vaginal discharge. The patient vomited frequently.

Necropsy. "Brain, 1150 grams. The cerebrospinal fluid is clear and in great excess. The brain is rather small but is not shrunken and is not adherent. Pia

mater is easily detachable disclosing a rather pale cortical surface. There is some general clouding of the meninges but no remarkable congestion. A few atheromatous patches are noted along the course of the main cerebral vessels." Microscopically there was atrophy of the cortex with an increase in the number of vessels, and perivascular infiltration by plasma cells. The glia was increased in amount and the ependymal lining of the ventricles was granular.

The peritoneum was found to be the seat of a very extensive metastatic carcinoma probably originating in the body of the uterus. The growth had metastasized to the lymph nodes, kidneys, suprarenals and surface of the liver. Microscopically it proved to be adenocarcinoma.

CASE 6. Colored male, 49 years old. (Aut. No. 4680, Dec. 28, 1924, pathologist, Freeman.)
Clinical diagnosis: General paralysis; lobar pneumonia.

History. Patient was admitted in May, 1924, confused, tremulous, ataxic. The pupils were fixed, Romberg sign positive, speech markedly slurred. There was a double murmur at the aortic area. The serology was strongly positive. He died after a short period of fever, with signs of pulmonary consolidation.

Necropsy. Death was due to a widespread confluent lobular pneumonia. The heart and aorta, however, presented nothing unusual and the clinical findings were unexplained. The brain was the seat of well-marked paretic changes, and of a number of infarcted areas in the basal ganglia and white matter. There was no perceptible enlargement of the pituitary body, but microscopically there was seen a localized proliferation of the chromophobe elements with large hyperchromatic nuclei and loss of normal architectural pattern that was interpreted as adenocarcinoma.

CASE 7. White male, aged 57 years. (Aut. No. 4842. Nov. 12, 1925, pathologist, Freeman.)
Clinical diagnosis: General paralysis; bronchopneumonia.

History. Patient had a chancre in 1901, and the onset of mental symptoms was noted when he attempted suicide in March, 1924.

Upon admission he was depressed, had self-accusatory delusions, loss of memory, and the physical signs of paresis. The laboratory findings agreed with the clinical diagnosis. A trophic ulcer developed over the sacrum, emaciation was rapid, and death resulted from an intercurrent pulmonary affection.

Necropsy. Both gross and microscopic findings were in agreement on paresis, although there was slightly more emphasis upon the vascular changes. The greater curvature of the stomach was the seat of a stellate growth, depressed in the center, with little extension beyond the mucosa, and no metastasis to the regional lymph nodes or liver.

Microscopically there was great hyperplasia of fibrous tissue of rather dense hyaline aspect with perivascular infiltrations that were at first suspected of being syphilitic. However, in the depths of the tissue were found large oval hyperchromatic cells of epithelial origin so that a final diagnosis was made of colloid carcinoma.

CASE 8. White male, aged 32 years. (Aut. No. 5022. Sept. 26, 1926, pathologist, Freeman.)
Clinical diagnosis: General paralysis (malaria); Bronchopneumonia.

History. Patient was admitted from the naval hospital a month after onset. Before admission he had become forgetful and inefficient, seemed confused and voiced some vague persecutory delusions.

There was slurring of speech, tremors of the face and lips, and other neurologic signs of paresis. The laboratory findings were characteristic. He was inoculated with malaria, had ten chills, after which quinine was given. He failed to improve clinically and a year later the malaria was repeated. After 14 chills he became suddenly much weakened and died in a few hours.

Necropsy. Death was considered to be due to malaria though there was beginning bronchopneumonia. The brain showed no inflammatory manifestations, but only gliosis and some reduction in the number of ganglion cells. A diagnosis of healed paresis was made. The pancreas appeared somewhat fibrosed at the time of necropsy, but no growths were seen.

In the microscopic section, however, there was a "small rounded mass, strictly limited, of irregularly growing cells in a matrix of connective tissue, the cells having a very hyperplastic and irregular disordered appearance." The diagnosis was adenocarcinoma.

Multiple Cancers

In the study of the biology of cancer, the occurrence of multiple cancers in the same individual, either simultaneously or successively, is of interest. In order to comply with the definition of "multiple cancers" there must, according to Orr,[11] be two or more essentially different primary malignant tumors in the same individual, although it is not necessary to have metastases from each. Therefore such multicentric tumors as those occurring in the skin, ovaries, etc., are to be excluded. Three instances of multiple centers of growth in the brain have been encountered.

Orr has collected the statistics of a number of authors, finding that out of a total of 9107 cases of malignant disease, multiple primary tumors have occurred in 76 cases or 0.83 per cent. Egli and Harbitz, however, reported 2.22 per cent and 3.05 per cent respectively. Orr himself, summarizing 7207 necropsies, found malignancy in 1046, and multiple tumors in 8 cases. He concludes that there is no etiological relationship between such tumors and that they occur purely as a result of coincidence. Nevertheless, it appears to be a strange coincidence that

[11] Orr, J. W.: Multiple malignant neoplasms, *J. Path. and Bact.* 33: 283 (April) 1930. (Quotes other authors.)

two separate primary malignant tumors should arise at the same time in the same individual. Moreover the appearance of triple growths as reported by Götting (pharynx, rectum, stomach) and by Walter (cardia, uterus, breast) and the appearance of successive growths as in the case reported by Gigl [12] should lead one to think in terms of a cancerous diathesis. The development of a sarcoma in a carcinoma of the breast should be referred to hormonal stimulation by the primary growth rather than to the innate tendency to develop carcinoma. The statistics derived from the intensive study of Madlener [13] indicating multiplicity in 14 per cent to 27 per cent of all cancer cases is worthy of notice.

My own data, based upon about 1000 necropsies, are given below. In these cases there is sufficiently detailed examination of the smaller

TABLE 8

Multiple Primary Malignancies

AUTOPSY	SEX	RACE	AGE	FIRST GROWTH	SECOND GROWTH	THIRD GROWTH
4612	M	W	59	Face	Stomach	
4666	M	W	68	Liver	Testis	
4893	M	C	46	Sigmoid	Pituitary	
4983	F	W	86	Gall Bladder	Pancreas	
5013	F	C	61	Pancreas	Pituitary	
5111	M	W	59	Parotid	Pituitary	
5134	M	W	70	Prostate	Lung	
5148	M	W	80	Prostate	Pituitary	
5101	F	C	80	Parovarium	Lymph node (metastatic squamous)	
5272	M	C	85	Prostate	Sympathicoblastoma	
5278	M	W	65	Stomach	Prostate	
5423	M	W	50	Cecum	Prostate	
5503	M	C	64	Sarcoma of back	Prostate	Pituitary
5511	M	W	67	Stomach	Sigmoid	
5565	M	W	77	Rectum	Prostate	
5576	M	W	54	Stomach	Prostate	
5630	M	W	54	Choroid	Pituitary	

[12] Gigl, J.: Ein Fall von zeitlich verschiedenem Auftreten von Carcinom, *Med. Klin.* 23: 432 (March 25) 1927.

[13] Madlener, Zur primären Multiplizität der Karzinome. *Deut. Ztschr. f. Chir.* 221: 1, 1929.

organs as well as the larger, so that a considerable number of cases that otherwise might have passed unnoticed, have been picked up. The requirements of the definition have been rigidly observed.

In 1155 necropsies performed during the past seven years there have been found 110 cases of malignancy, about 9.5 per cent. In these 110 cases there have been found 17 cases of multiple primary tumors, an incidence of 15.5 per cent among the cancer cases and 1.5 per cent of all patients submitted to necropsy.

Applying the formula for determining the probability of occurrence, we find:

Autopsies	1155
Malignancies	110

Multiple Malignancies		
Calculated	Observed	Percentage
10.5	17	162

From this it appears that the occurrence of multiple malignant tumors in the same individual is more than a coincidence. Moreover, a considerable number of border-line cases, in which the malignancy of the second tumor was open to doubt, have been excluded. There would therefore appear to be some definite relationship, etiologically speaking, between the two primary tumors. This is particularly to be stressed, since the two neoplasms are present at the same time, usually in persons considerably past middle age.

Two explanations are possible to explain the occurrence of multiple cancers, since the observed incidence is so much higher than the calculated.

Either one of the tumors may give off into the blood substances that excite disorderly proliferation in the tissues of another organ, or there is some fundamental disorder in the process of senescence that permits certain tissues to undergo malignant hyperplasia. The recent article by Warthin [14] dealing with human inheritance of carcinoma explains the occurrence and localization of malignant tumors on the basis not only of inherited general factors, but also of organ specificity. The difficulties of investigating this in our material are self-apparent.

An objection to the first explanation, that of distant hormonal stimulation, is the infrequency with which primary tumors arise in the

[14] Warthin, A. S.: Heredity of cancer in man. *Ann. Int. Med.* 4: 681, 1931.

liver and lung, etc., in conjunction with metastatic tumors; another is that the growths are often so small, even microscopic in size, or so equivalent as to development, that the influence of one upon the other is highly problematical. One is led to conclude, therefore, that the inherent tendency to the development of carcinoma is emphasized by the high incidence of simultaneous multiple growths.

It would be rash to expect much information on the cancer problem from the small material available here, but the data would not be complete without some consideration of the treatment of malignant disease in St. Elizabeth's Hospital. Of 59 cases of malignant disease coming to necropsy from 1925 to 1929, seventeen had been submitted to surgical operation before death, and eight had had intensive radiotherapy following operation. Relatively few patients have come to necropsy following operations for cancer without showing recurrence or metastasis. Such cases are detailed in Table 9.

TABLE 9

Successful Operations for Cancer, with Subsequent Necropsy

AUT. NO.	AGE	SEX	RACE	PRIMARY GROWTH	DURATION OF LIFE	CAUSE OF DEATH
893	74	F	W	Breast	Unknown	Arteriosclerosis
1758	65	F	W	Breast	Unknown	Tuberculosis
4066	68	M	W	Hand	3 years	Arteriosclerosis
4365	71	M	W	Testicle	4 months	Cer. hemorrhage
4397	60	F	W	Breast	14 months	Cholecystitis
4923	77	F	W	Arm	5 years	Pneumonia
5391	75	F	W	Breast	14 years	Cholecystitis

This does not give an altogether exact picture of the treatment of malignant disease at the present time, since only in recent years has an active campaign been waged against cancer. In 1928 a survey of the breasts of females revealed a number of early cases of carcinoma, and most of these were submitted successfully to operation. Moreover, other patients have died following operation, and without visible recurrence, but have not come to necropsy, and a few have been discharged from the institution following operation. On the whole, nevertheless, the outlook for a satisfactory handling of malignant disease in this institution is far from hopeful. Even the keenest internist is often baffled by the lack of coöperation on the part of the patient in the examination, and the surgeon has to reckon with the tendency on the

part of certain patients to tear the dressings from the wound, and even to pull apart the edge of the wound. Deep radiation has been unsuccessful in deep-seated growths. The only hope seems to be in closer medical supervision of the patients, with periodic surveys of certain organs, such as the breast survey noted above.

SUMMARY AND CONCLUSIONS

Tuberculosis, syphilis and cancer are important factors in the high death rate among the psychotic population. Tuberculosis and syphilis, however, have yielded considerably with the introduction of energetic measures. The isolation of the tuberculous and periodic temperature surveys on the psychiatric services have almost brought this scourge under control. Furthermore syphilis, at least in the white race, is rapidly being brought under equally good control by means of the newer fever therapy, and the recognition of the disease is made ever more certain by the use of multiple serologic tests. Cancer may be increasing although the figures are not conclusive. Less than 30 per cent are operated upon before death, an equal proportion escaping recognition before necropsy, and the rest being recognized as inoperable for one reason or another. The success of a breast survey in detecting early cases of malignant disease should encourage the organization of similar surveys, analogous to the temperature surveys for the detection of tuberculosis. The recognition of the importance of the psychological panel in diagnosis and prognosis should be of further value in this regard, since a majority of cancers develop in individuals of cycloid or paranoid behavior, and a great majority of the tuberculous fall into the schizoid class. It would seem possible to apply such diagnostic and preventive measures to the groups to which they are best adapted.

The antagonism indicated in statistical fashion between separate disease entities such as tuberculosis, syphilis and cancer is believed to rest upon fundamental biologic reaction patterns whose psychological correlates are recognized by the psychiatrist, and not upon some inherent incompatibility in the chemical or microbic agents. The unexpectedly high incidence of multiple cancers is an argument in favor of the blastomatous nature of malignant disease, and bears out the ancient impression of a cancerous diathesis, an inherited tendency.

ANTHROPOLOGICAL APPROACH TO ETHNOGENICS*

A NEW PERSPECTIVE

BY GEORGE PITT-RIVERS

In his 1931 Presidential Address to the Royal Anthropological Institute on "Anthropology, pure and applied," Professor Myres discussed in retrospect and prospect the proper functions and legitimate aims of the Institute. On previous occasions he has touched on the place of anthropological studies among other branches of learning, and on the claims made from time to time that anthropological research has positive contributions to make to national well-being.

If I encroach again from a somewhat different angle on ground well worn, my excuse must be that anthropological themes seem to lose their newness as little as anthropology itself appears capable of losing the title, which it has undisputedly held for at least the past sixty years, of being a new science. In the year, 1931, Dr. Myres admits that "old prejudices against a new subject like anthropology die hard . . . . and systematic provision for anthropological studies, like Anthropology itself, which is still a very young science, is in its infancy."

Should we suspect that an infant which remains for sixty years in its early infancy is arrested in its development? And if it is arrested should we suspect lack of nourishment?

I do not wish to anticipate the answer, but the question remains implicit in what I have to say. Speaking in 1908, on "The Scope of Social Anthropology," Sir James Frazer called his subject "comparatively new and its limits still somewhat vague." In the intervening twenty-three years between then and now, Social Anthropology has claimed increasing, though still very inadequate, attention in our Universities, by the establishment of Anthropological Departments in some

* Revised and amplified from a paper read before the Anthropology Section of the British Association, London, 1931. The term 'Ethnogenics' here replaces the term 'Eugenics.'

of our Dominions and Colonial Dependencies, and by the increasing volume of learned works in many departments of the subject.

If I venture to echo that comment of our great and revered pioneer folklorist and social anthropologist again to-day, it is not because I ignore all the work that has been done. Yet I am as tempted to repeat the excuse, or accusation, of 1908 and 1931 with even greater emphasis: the subject is still comparatively new and its limits and scope perhaps vaguer than ever they were before.

THE NEED FOR APPLIED ANTHROPOLOGY

The aim of Social Anthropology remains, consistent with the aim of every other science, to discover the general laws to which particular facts conform and by which alone they can properly be explained. In short, it aims at explaining what has happened and what is happening in the regulation of human history, irrespective of time or place. Consequently it must be prepared to be some guide as to how we may best conform to what may happen in the future—or our profession may be regarded as an amiable diversion of no great importance in the affairs of everyday life.

May I then not yet claim that the scope of Social Anthropology can no longer be restricted as it was by Frazer, and is still, I think, by Dr. Myres, to the rudimentary phases of human society? That in spite of the limitations the former sets to the boundaries of his special studies, we can no longer avoid concern with the practical application of its results?

The pursuit of our scientific researches into man's social history involves a diagnosis, which leads to a demand for a prognosis; hence to an Applied Science. Applied Anthropology has come into being; not even the clumsy and antiquated terminology by which Social Anthropology is supposed to be distinguished from Sociology can disguise that fact. Some of our French anthropological colleagues at any rate do not attempt the distinction, and the school of Emile Durkheim and Marcel Mauss call themselves sociologists or anthropologists indifferently; neither do they restrict themselves to the rudimentary phases of human society.

If Anthropology is still a comparatively new science, and we seem to infer it chiefly by the relatively slight influence that that discipline appears to exercise upon human affairs, and the very slow growth of any discernible influence it exerts upon human opinion, it is remarkable

that it should have remained in its infancy for such a very long period. How is it that we are still discussing at its cradle, in much the same terms, such an early post-natal subject as its future career and development—its scope and methodology?

I may go back, not twenty-three years, but more than twice that time, fifty-six years, and invite your attention to the discussion of the same subject by a former President of the Anthropological Institute. It is, I think, no philo-progenitorial prejudice which makes me think my grandfather's Presidential Address in 1876 still curiously relevant.* It is perhaps a pity that it is so. The terminology that had come to be employed in the classification of our subjects he found at least as unsatisfactory as I venture to think it is to-day.

The term Anthropology, which he wished to reserve as the most embracing term to cover all Human Biology and Sociology with their respective sub-divisions, together corresponding to Constitution and Culture, was then customarily divided into the four unsatisfactory departments of Ethnology, Archaeology, Anthropology proper, and general works relating to Ethnological subjects.

If we are, after this very long interval, still willing to encumber ourselves with such a classification, the empirical, inductive, and the applied and dependent branches of our sciences will become increasingly divorced, and to some extent sterilized and narrowed in outlook. Some were included as our proper study: Anthropology proper. Others, as essential to the proper study of man, were driven to seek the shelter of other labels and different societies: and were presumably exiled as Anthropology "improper."

I wish, here, to do no more than suggest that the primary classification of anthropological *subjects,* instead of the *classification by classes and methods of workers,* may have been as injurious to Anthropology as the polygenetic search for a fixed number of primary human races. Were it not for this, would not Eugenics, Genetics, Demography and the study of populations, and of Psychology, have their recognized places as branches of Anthropology?

It is at least interesting to note that amongst the papers read before the Anthropological Institute in 1875, we find four papers by Francis Galton dealing with subjects now classified under Genetics or Eugenics.

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* Address to the Anthropological Institute of Great Britain and Ireland, on 25th Jan. 1876, by Colonel A. Lane Fox (afterwards Lt. General Lane Fox Pitt-Rivers), President of the Institute.

They include: A History of Twins; Heredity in Twins; The Height and Weight of Boys in Town and Country Schools. The same President in his summary observes: "It is to be hoped we shall not have to wait until the principle of heredity has asserted itself in Mr. Galton's offspring before we have other contributors to this important branch of our studies." In addition we also find a paper on Comparative Psychology by Herbert Spencer, and a study of Longevity by Sir Duncan Gibb. The infant science had plenty of fresh nourishment in those days.

All science has its birth in man's desire to control his environment or to adapt his needs to its forces by seeking to understand them. The early Polynesian navigators sailing in their frail canoes across the Pacific, owed their lives to the stars and became astronomers, and they became anatomists in the school of anthropophagy because they found human flesh sweet to eat.

No science can long retain the interest nor spur the labors of its devotees, let alone of its public, unless it can deliver the goods. It must continually supply answers to the ceaseless triple question whereby man seeks illumination along the path of survival: "What is happening; why is it happening; then what should we do?" For the answers that science may give range from replies to the first question to the last, and it is the last question which prompts the enquiries which furnish an answer to the first. By the time we begin to furnish answers to the last question "What should we do?", we have formulated an Applied Science. A clear vision of its applications is the best discipline in the methodology of a science. Before we can observe well, we must know what to look for and why we are looking.

On these grounds, I venture to think, Francis Galton has a foremost place amongst the greatest methodologists of his age, and a place that no mere empiricist can aspire to share. For this reason, too, we must place the science he named Eugenics first in the list of Applied Anthropologies. But the scope of Eugenics is far wider than that usually accorded to it: this is not surprizing since the applications of Anthropology have so very recently begun to receive general attention.

It is curious to reflect that in respect of time, far less methodical study has been applied to gaining a knowledge of the functioning, origin and nature of human institutions, of the decay and survival of races, or of the laws governing the genetical transmission of human qualities and defects than, for instance, has been applied to the study of zoology

and its practical applications in the breeding of animals and in farming, and still less to making that knowledge widely known.

CONSTITUTION AND CULTURE

The problems of Eugenics are co-extensive with the problems of Anthropology. As soon as we begin to diagnose the factors controlling the changes, the increase, decline and substitution of types in the populations and races of the world, we find that constitution and culture are inextricably mixed. For evolution is a process of adaptation, physical, mental and cultural, to environmental conditions.

The progress of Anthropology must therefore increasingly depend upon the re-synthesis of all its dependent branches, biological and cultural, that together mark the changes, evolutionary and adaptive, which describe man, singly and in his social setting. The science embraces the animal man and his environment in human populations and the link between the two, for his surroundings are continually provoking from him new reactions and fresh adaptations. The reactions are cultural, but the changes in time from generation to generation are constitutional and may be measured in the different rates of achievement in adaptation; rates of the elimination and of the survival of types. Here Anthropology must seek its verifications in the demographic study of populations.[1] But, as Dr. Georg Thilenius puts it: man, the subject of active and passive adaptation to fresh surroundings, is further determined by his hereditary characteristics, and is paratypically influenced by his environment. Thus both culture and heredity determine his constitution: both are factors in mutation and selection.

Somatic, cultural and psychic phenomena are thus interdependently linked in the study of the time changes in man. From this point of view arises the newer conception of the study of race, and race changes, or, in the phrase of Sir Arthur Keith, of "race formation." Dr. Thilenius implies a similar dynamic conception of race when he says: "To the conception of forms was essentially added the further conception with regard to their distribution and possible temporal limitation." "To-day we share Scheidt's view, based on the biological conception that it is fundamentally necessary to examine, not merely selected sections of the

[1] The study of the laws of racial adaptability is the main thesis of my book "The Clash of Culture and the Contact of Races," where the conception of variational type adaptation and its implications is first developed.

nation, but the whole regional population, in its entirety." And furthermore, "Every culture is bound up with its human representatives."[2]

From this point of view we approach Anthropology as the biocultural history of man, and it comprehends also an aetiology of the change or movement within a nation. Or it may be described as the history of populations—regionally considered—inter-related to the history of races, or the migrations and changes of stocks—which again is related to the history of culture and its evolution. Methodologically we must distinguish this treatment of phenomena, its observation, classification and aetiology, from its applications. In turning to account the study of anthropological phenomena, so defined, we approach its Eugenical bearings. This will be a wider conception of Eugenics than the traditional one by as much as the scope and direction of Anthropological aims will also have been widened.

In so far as Eugenics transcends as an applied science, perhaps inevitably, the rigid limitations of an exact prognosis of the human races' changing forms, features and morbidities, by an insistent demand for the satisfaction of human aspirations, ideals and biassed hopes, so far Eugenics must remain, strictly speaking, unscientific. To that extent, too, Eugenics must retain something, as Galton himself maintained, essentially akin to the enthusiasm of a religious hope or faith. To that extent also it will admit of both the strength and weakness of religious conviction: the strength that leads ardent striving after an ideal towards eventual realization in fact and achievement, and the weakness that tempts its devotees to mistake the wish world of imagination for reality. In such terms Nietzsche's Zarathustra voiced his dynamic but essentially ethical and non-scientific challenge: "Not only onward in time, but upward shouldst thou propagate thyself! For that purpose may the garden of marriage help thee."

ETHNOGENICS

Nietzsche, profound as was his sociological insight, scorned a methodological approach; but his time was not ripe for a methodology of the social-biological sciences. At that time Eugenics was born as an applied science of the future. The term as Galton coined it, with the value-implication of the first syllable, was no more than an anticipation of a

[2] Georg Thilenius, "On some biological view-points in ethnology." (The Huxley Memorial Lecture for 1931.) *Journal of the Royal Anthropological Institute*, Vol. LXI, July-December, 1931.

riper body of knowledge and method, to be achieved after much labor and study. The time should now, however, be ripe for a new scientific synthesis, with a defined and surer method. Out of it there emerges the conception of race, population and culture as tripartite aspects of man in time, conditioning and being conditioned by his environment: the conception of race in evolution. To distinguish it from these earlier anticipations it should be profitable to introduce a newer term; a term which briefly indicates the implications of race-population change: ETHNOGENICS. The term has no propagandist coloring, for the ethnogenist is concerned with an exact prognosis of race, population and culture change, and the aetiology of that change in the past.

The new synthesis will work for a correlation between somatic and psychical forms, and on the physiological side for a standardization of criteria similar to past ground-work on the side of physical measurement. Such detailed work, for instance, is already being carried on, notably by Professor Elton Mayo, in the Industrial Research Institute of Harvard University; work which correlates psychological and physiological indices, and which points to striking familial and group differences in physiological capacity; work which contributes to a newer exact ethno-sociology or ethnogenics with statistical verifications. Unlike the frequent tendency in past eugenic discussion, the use of the term 'desirable,' unrelated to some measurable distinction in regard to any race or racial quality is, from this point of view, deprecated; for ethnogenists will prefer to replace it in terms of adaptation.

FACTORS IN THE DECLINE OF RACES

Little support is now given to theories based on the once-supposed fixity of racial types. But we have barely begun to formulate the laws underlying the decline and the increase of populations, and the variations in adaptability exhibited by different race types in contact. Certainly, I think, we can discount those facile generalizations implying that the causes of the extinction of certain primitive races are easily recognizable in some arbitrarily selected checks to population or factors of elimination.

Examination often shows no correlation between the rate of decline and the incidence of selected mortality rates. A declining ethnic type or group often shows a low mortality rate. The approach to the problem is too often wrong, because it is forgotten that we are studying variational adaptation, and verification depends, as I have already said, upon

a proper use of demographic data; and demography is the most neglected branch of Anthropology. Races and types die out because of inner causes, provoked often by outer changes. Progressive loss of viability and extinction of human race types occur, as biologists and paleontologists know in the zoological field, when viability or growth forces fail to respond in adjustment to environment.[3]

This maladjustment may result from overspecialization, which tends to make adaptation to variations in external conditions more difficult and more rigid. But specialization and overspecialization, while conditions of racial old age, are quite different from degeneration, so that changed conditions of environment often enable a more generalized and mixed type to replace a purer, more homogeneous, more specialized and highly developed type. Race mixture or miscegenation thus plays a part in the survival and the selective elimination of types.

One of the populations I have used to illustrate the operation of this process of variable adaptability is the Maori of New Zealand, where the full-blood population declined rapidly, after impact with European civilization had upset the equilibrium of adjustment with a stable environment. The rise of a new hybridized population of mixed European-Maori blood effected and is still effecting a gradual substitution of the old stock by a new, less specialized and more adaptable one. These two curves of population show opposite tendencies; the significance of the contrast has still been insufficiently appreciated.

While this demographic verification appears to relate primarily to constitution, culture no less is involved in the diagnosis. For environmental factors are equally cultural, and the mechanism of adaptation, (that is, the reaction to changing conditions,) is a process expressible in psychological terms. Since we are dealing with innate variations of type, we touch on the subjects of Genetics and Eugenics.

A part of my verification involves an incursion into the specialized study of sex ratio variation. Here it must be sufficient to record the results of a very wide investigation, which shows a correlation between fluctuations in the balance of the sexes and an increasing or decreasing population. This variation is more marked in the reproductive age categories than the variation at birth, and is therefore brought about largely by fluctuations in the differential sex survival rate. A large percentage of males amongst the young has also been noted as a symp-

[3] Cf. I. P. Tolmachoff. Extinction and Extermination. *Smithsonian Report for 1929*, pp. 269-284.

tom of a declining stock.[4] Increase in masculinity is correlated with a decreasing curve of population, increase in femininity with an increasing curve. Since the influence of miscegenation is reflected in the rise or fall of population, it also shows in contrasted sex ratios of the full blood and mixed blood stocks of populations like the Maori. To-day, Australian aboriginal and Red Indian populations furnish significant examples. Other demographic features reflect the sensitiveness of the social structure to the force of these laws, such as differential variation in the respective nuptial ages of men and women, and postponement in the male or female nuptial age.

These are problems within the scope of Anthropology, of Demography and of Eugenics; they constitute the material for ethnogenic research. For all quantitative variations in population and the factors which produce them effect also qualitative changes, most conspicuous when populations are least homogeneous. Elimination and survival are selective, and evolutionary change works through variable adaptation. Inbreeding also makes for the segregation of distinctive types, while on the other hand, outbreeding militates against the specialization of types. Social customs such as cross-cousin marriage and endogamy play a part here. The inbred Fijian chieftain families showed conspicuous variations in terms of viability, survival, fertility and other measurable distinctions. Before the catastrophe of their impact with European civilization the inbred families were superior in these respects; afterwards they ceased to be so. The effects of monandrous, polyandrous and polygynous matings may also be analysed by these criteria.

SOCIAL DISINTEGRATION IN ENGLAND AND AMERICA

But all these problems may equally well be studied at home among our own European populations; qualitative as well as quantitative changes are taking place before our eyes. There is ample work for an army of specialists, but there is still a dearth of workers in the most vital yet neglected fields. Depopulation and over-population are phenomena reflecting the same laws and processes which equally bring qualitative changes with the substitution of types and stocks. But surviving stocks are sometimes not of the type and quality capable of carrying the burden of the complex civilization created by the original stock which gave it its peculiar form. The industrial revolution in

[4] Cf. I. P. Tolmachoff. *Idem.*

England has been an environmental revolution carried along at an increasing rate of complexity by its own momentum, bringing in its train a vast increase of population which it threatens to find increasingly difficult to absorb profitably or healthily. The progress in technical efficiency in the development of machinery has outrun efficiency in the control and organization of the human material whose labor it absorbs. Here we touch the most pressing economic problem of the day. The increasing use of machinery has tended to reduce the amount of labor needed to man the machines, with the consequence that the increased output of machine-made goods finds a decreasing human market for them. The machine has outstripped man in its development. But this grave economic depression, vaguely referred to as world economic depression, has hardly been recognized at all for what it is—a cultural and a population question. The very remedies proposed, such as the plea for the 'rationalization of industry,' which means ultimately the use of more machinery and less labor, without tackling the problem of the absorption of labor, are but aggravators of the causes of the trouble. At the same time a differential survival rate is producing a change in the composition of our populations. The problem of the clash of cultures in Europe still awaits scientific investigation.

Adaptation, the price of survival, is no longer the simpler problem it was for our leisurely increasing forbears of the fifteenth century, when England had a self-supporting, mainly agricultural population of some three millions. Science, the aggregated product of a few men's brains, has produced the vast machinery which has changed the face of our world and every minute of our daily life. Do we clearly know what sort of man the machinery has produced, and is likely to produce in the future? And what type of man is best fitted to survive in such an environment? And which type is doomed to disappear? And with his disappearance may not our civilization itself go with him?

Civilization increasingly requires in its growth and sustenance variety in the men by which its machinery functions, and if the controlling factors of our changing environment operate selectively but unevenly, there seems to be evidence that they may be developing in different proportions incompatible types of men, increasingly alien to each other and incapable of the co-operative productiveness that alone ensures the cohesion, strength and health of a nation.

This problem is beginning to force itself upon the attention of some American investigators; for symptoms of social disintegration are showing in the fabric of society and in its industrial life. From the racial melting-pot emerge groups intractably drawn into the economic life of the country from which they tend to withhold any collective wholehearted allegiance. Their dawning self-consciousness is colored far less by any economic stratifications than by their ethnic heritage, and by the clash of incompatible culture trends. There are negro groups, Indian groups, southern white groups, and regionally scattered groups, preserving the tongues of their diverse European heritage.

Though perhaps less conspicuous than in the new world of America, there are symptoms of social disintegration in England, in perhaps a different form, of the same disease that destroys societies and brings about the collapse of civilizations and cultures. I have described Science as the conscious and systematic attempt of man to control his environment, the conscious part of adaptation in his fight for survival. But by far the greatest and most imposing advances in science have been made in man's control over the physical forces of his environment; on the organic and the cultural sides he has lagged most sadly behind. Just where there is the greatest need for a science of the control of society, in its organization and composition—quantitative and qualitative—it has not been allowed or has not been able to help.

It is not only in our colonial Empire that the applications of Anthropology and Ethnogenics can help us to avoid disasters, promote efficiency and effect economies; many of the problems customarily looked upon as essentially economic or political can be elucidated only by the help of careful anthropological investigations and method. For instance, the long duration of unemployment in England since the war, and the appalling drain on the national resources involved in maintaining the unabsorbed army of our industrial life, is more than a transient problem of temporary dislocation. That it is not merely an economic but also a population problem, in which the root causes are to be elucidated by anthropological methods and by the application of scientific insight is not generally appreciated.

When it is considered that social and economic legislation, which is determined by political exigencies in ignorance of the consequences of the influences at work, directly affects most of these factors, the importance of the right scientific training and knowledge becomes apparent. By no other means can we hope to make the world safe for intelligence.

In 1923 I was privileged to make the Presidential address to the Anthropology Section of the Australasian Association, and I took as my theme "The Disintegration of Tribal Communities," under the subject-heading "Mental Anthropology—the Science of Civilization." I attempted to show that the stability and social health of any community, whether a tribe or a nation, however high or low in the scale of culture or complexity, may be reckoned by the degree of integration or disintegration it exhibits, and that every weakening of the tribal tie destroys the social purpose of each member of it. The disintegrative symptoms may even be shown and measured in such ways as the suicide rate, as Durkheim showed in his classic study of suicide, or by the conflict of industrial, civil and religious factions. I then enumerated some of the functions by which society achieved its cohesion.

Since that time, in later work, my studies have led me to consider how social disintegration affects and is linked up with changes both quantitative and qualitative in a population. It was therefore with interest and gratification that I read a Presidential Address by Professor Radcliffe-Brown [5] before the Anthropology Section of the Australasian Association in 1930, and to find that he also took as his theme in the domain of Applied Anthropology the "Disintegration of Tribal Communities." My confidence in my own early essay, seven years before, was very greatly enhanced by the endorsement it appeared to receive from Professor Radcliffe-Brown's own treatment and by his conclusions. I could find, in fact, only one point in a single sentence from which to differ. But as that point concerns the theme of my present paper I venture to mention it. Our difference is, I conceive, more verbal than real.

He said: "Attempts are being made to institute generally under the name of Eugenics, an Applied Science of Human Biology. These attempts are, to my mind, premature." He admitted the desirability of taking those steps as soon as possible, but added: "Our theoretical knowledge of the phenomena of heredity and variation is totally inadequate at the present time to take up the very difficult task . . ."

Premature! Do we not already know far more than sufficient, or suitable, to teach the teachers of the coming generation the essential, though elementary, facts about themselves, which the present generation ignores or of which it is ignorant?

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[5] President of Section H (Anthropology), British Association Centenary Meeting, 1931.

I conceive that I have in anticipation answered this previous objection. Professor Radcliffe-Brown has already gone with me so far in allotting to the scope of Applied Anthropology the task of formulating a doctrine applicable to the control and direction of social development and the art of Government. But I have already tried to show that it is impossible to divorce an Applied Social Anthropology from an Applied Human Biology. Human Biology thus has a concern in all the influences, cultural, physical and environmental, that affect the extinction or the survival of ethnic variations or types. Quantitative changes bring about qualitative changes. In so far as these influences are amenable to social control or direction, they come within the domain of an Applied Science. Ethnogenics is therefore the study of those forces, amenable to social control, which may influence the fertility and survival rate of variations of type in a population: it is a necessary aspect of that 'functional anthropology,' which has two distinguished exponents in Dr. Malinowski and Professor Radcliffe-Brown.

We are often reminded that the backward aboriginal races of the world are fast disappearing, as an urge to speed our investigations of their cultures before our material is lost forever, but ethnic types may disappear amongst thriving and dense populations, even here in our midst. Are we less urgently concerned in preserving those types and those races which enrich our civilization than in writing with academic precision, the obituary notices of those most remote from us; or are eschatology and the measurement of skulls the only really important branches of Anthropology?

POPULATION STATISTICS IN THE KINGDOM OF DAHOMEY [1]

BY MELVILLE J. HERSKOVITS

I

Students of primitive civilisations who have carried on their investigations among peoples having large populations, have often described well-developed political systems, giving in great detail the outward manifestations of this phase of such cultures. Especially is this true in the case of the West African peoples, where were found kingdoms numbering hundreds of thousands or even millions of subjects, governed by efficient, well-functioning monarchies. An example of such a people is had in the Dahomeans, for nowhere in West Africa, perhaps, was the reigning house more firmly established than in Dahomey. Beginning with the early seventeenth century, the Alladahonu dynasty maintained itself on the throne with ever-increasing power until the overthrow of the kingdom by the French in 1892. To read contemporary accounts written by those who visited the kingdom while it was under native rule [2] is to be impressed, as these travellers were impressed, with the monarchy and the efficiency of its operation. Despotic in the extreme, and determined in its purpose, the strength of its organisation is always commented upon in these accounts. There were systems of customs houses which not only taxed produce and articles of trade transported from one part of the kingdom to another, but also controlled

[1] The data contained in this paper were collected during the Columbia-Northwestern Universities Expedition to West Africa, 1931. I wish to express my appreciation to my native friends whose aid made it possible to obtain the material presented here. With the destruction of the monarchy, the customs I describe no longer obtain, but there is every reason to believe that the system of counting the population was essentially as outlined.

[2] As, e. g., those of Sir Richard F. Burton, who visited Glele in 1863, and wrote "A Mission to Gelele, King of Dahomey," or of J. A. Skertchly, who spent eight months in Abomey in 1871, telling of his observations and experiences in his volume "Dahomey as it Really Is."

population movements. The intricacies of court etiquette and the lavishness of display that went with the ceremonials of state, implied a large revenue for the support of the court and of state ritual. And that the early accounts had not overrated the power and efficiency of the Dahomean political organisation was perfectly apparent when, in the course of field investigations there, the life of Dahomey was studied. Even today, it can be seen to what extent every phase of existence was touched by the power of the monarchy. The word of the king was law to his chiefs, and the word of the chief was law to those under him. The ideology which made possible this discipline forms a part of the religious concepts of the people; for the realm of the supernatural, as envisaged by them, has the same hierarchial organisation as the life of living human beings.

It was interesting, therefore, to investigate something of the inner working of one of these West African monarchies. The structure of these governments is well known, yet what were the mechanisms of control they employed? The data contained in this paper represent a portion of the information gained as a result of probing this inner organisation of the Dahomean monarchy. Whether or not there existed a similar type of organisation in other West African kingdoms cannot be said, for comparable material does not exist; so that the system of collecting and keeping population statistics described in this paper may perhaps be a unique one developed in Dahomey alone, or it may partake of political traditions that extend over all of West Africa. Whether this be the case or not is a question for field research on the part of investigators in other regions.[3]

II

When the elaborate funeral ceremonies for a deceased Dahomean king had ended, the enstoolment of the new king achieved, and he was securely vested in his power, he made a tour of the grounds

[3] It is difficult to give an estimate of the population of Dahomey under its native rulers, for, as will be seen in this paper, the counts made before the conquest were so secret that it would be impossible to obtain the figure even today. Skertchly, a careful observer, estimates it at between 150,000 and 180,000, but I feel this figure is far too low, as he visited neither Adja to the west of Abomey nor the region toward the Weme river to the east. The 1931 census, taken by native officials and said by M. Poiret, Commandant of the Cercle of Abomey, to be an understatement of the actual number of people, credited the Cercle of Abomey alone with 83,645 persons.

within the palace walls. Inspecting house after house, he finally reached a long, low building with many doors. In front of it, as the king approached, two officials were standing, and they caused all who accompanied the new monarch to retire. They were old men who had belonged to the entourage of the new king's father, and as they took him into the first room, they showed him sacks filled with pebbles, which represented the population of Dahomey,—men, women, and children—during the reign of Hwegbadja, who died in 1680. For every succeeding king, so the account goes, there was a similar chamber, containing similar sacks. As the two old men showed their new royal master through the rooms, they pointed out how the size of the sacks at the end of each reign was greater than those which represented the population of Dahomey at the end of the preceding king's tenure. They told him of the conquests of each of his ancestors, and how these had increased the number of people in the kingdom.

When they reached the rooms where lay the sacks of pebbles that denoted the population of Dahomey during the reign of his father, the two elders spoke to the new king: "Young man, kneel!" Then, as he knelt there,—"Young man, all your life you have heard Dahomey, Dahomey, but you have never until today seen the true Dahomey. For Dahomey is its people, and they are here." Then, pointing in turn to each of four sacks of white raphia with pebbles in them, they said, "In this sack is the number of men which today we put in your hands. Here is the number of women. Here the number of boys. Here the number of girls. You must never let these sacks become smaller; you must always increase the number of pebbles. You must consider how small are the sacks of Hwegbadja, and how great those your father has left you. Every year, we will come here with you, to count the pebbles, and see if you have increased the number of them, or to see if their number has lessened. Young man, rise! We have not given you this thought to discourage you."

They gave him a very old gun—tradition has it that in the time of Hwegbadja they gave him a hoe-handle, the weapon with which the Dahomeans are said to have fought in early times—and said, "Fight with this. But have care that you are not vanquished." Then they sang for him, songs which must not be sung outside this place, and told him to go; the year following they would see him once again.

III

The Dahomean kings did not obtain their information about the number of people, or any other phase of life in their kingdom, by direct inquiry. Just as to base a tax on information derived from direct questions as to the number of palm trees or sheep or goats a man possessed, or the number of hoes he turned out, or the number of animals a hunter killed, or how many loads a porter carried, would have been regarded as foolish since this would have led to falsification, so a census, as such, was never taken. A remark dropped by an informant, apropos of the method of obtaining the numbers of the people, reveals strikingly the Dahomean belief in the inadequacy of direct questioning as an administrative technique. When I remarked on this indirection, the reply came, "In the days of Hwegbadja, our people did not even have guns, and they had to fight with hoe-handles. They could not write. But they knew enough to get their information in this way."

The Dahomeans were enthusiastic soldiers. Before 1865, they were battering at the walls of Abeokuta, a city of over seventy thousand population situated in southern Nigeria some hundred miles east of Abomey, the capital of Dahomey. Not only were the men warriors, but it was in Dahomey that the battalions of women soldiers, of "Amazons," as they are termed in the literature, were found. And it was on the grounds of preparation for war that the king counted his people. The prime minister and chief executioner of the king, the Mingan, had command over the village-chiefs of the region of Abomey. Shortly before operations of a new campaign were to start, every head of a "collectivity," e. g., a group of compounds inhabited by those families related in the male line, brought an account of the number of his men to the head of the village, who placed a number of pebbles corresponding to the total in a small sack. In this count were included all males over the age of thirteen, because above this age, all were considered men.

About ten or twelve days before mobilisation was to commence, the village-chiefs, with their sacks of small stones, came before the Mingan. On each sack was sewn a symbol which indicated from which village it had come,[4] so that there would be no difficulty in identifying it. In the South, the villages about Whydah reported to the Yovogan, who there

[4] These devices—over two hundred of them for the district of Abomey—showed some characteristic of the village concerned. At Umbegame there was a great tree under which there was a temple, where swallows always were to be found; hence the sign of this village was a swallow. For Sinve, where baskets are

represented the king. In Allada there were five sub-chiefs to whom the chiefs of villages brought their sacks; in the north, two; to the east, two; and to the west, two. All the sacks sent to these district chiefs were also brought to the palace at Abomey, and any chief who disclosed the number of pebbles he brought to the palace did so at the peril of his life, for were this disclosure to be discovered he would lose his head. The essence of this system, as a matter of fact, was its secrecy. The power of Dahomey lay in the number of its inhabitants, and this constituted a secret which the king alone might know.

The sacks, then, were brought before the Mingan. But while it was his duty to receive their bearers, it was not his right to count their contents. This was done by an official called the Koto, who took the sacks, still in the possession of the village-heads of the district of Abomey, to the king himself. The chiefs were conducted by one of the king's wives to whom this service was specially delegated, and who, with the Koto, had already counted the pebbles and knew how many men were to be found in each village. The king, as each chief prostrated himself to show respect for the monarch, would indicate to which army corps the men from his village were to be assigned. Nothing was said as to the number of men who were to go. After the campaign, however, the commander of each corps would be asked how many men he had received from each village, and if the answer was not a number equal to one-half of the counters in that year's sack for that village, its chief was strangled.

What of the enumeration of the women? After the troops had been mobilised, there was a period before the army set out for the battle, during which organisation was perfected. When the men had been assembled for six days, every commander asked each of his soldiers how many wives he had, whether his mother was alive or dead, how many unmarried daughters over thirteen there were in his family. In accordance with the principle of indirection I have already enunciated, the real purpose for which this information was desired was not indicated, for the soldiers were told that the king wanted to know those who had been left at home, so that if a man were killed on a battle field, his dependents could be recompensed for their loss.

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made, the symbol was a basket. For Zumi, where clay is found, the device was a pot. Gboli harbored those who made charms, hence its symbol was a charm. The list could be lengthened, but these examples sufficiently show the principle of identification.

The army chief, using pebbles to indicate the number of women dependent on his soldiers, village by village, reported the number to the palace, where the sacks were put in charge of a woman who kept check on the affairs of the army. When this count was finished, the army began its campaign. On its completion, each commander was asked how many men he had received from each village, being confronted with the village-chiefs, each of whom, as a control, was asked also to tell the number of men who left for war from his village and the number who returned. At the same time, before a commission presided over by one of the war-chiefs, the heads of the army-corps confirmed the numbers of women and girls over thirteen reported by the men from each village. Thereupon, each village-chief was told to indicate the day on which he would again appear before this court with the heads of those collectivities from which men did not go to war.

It must be understood that every Dahomean man was regarded as a soldier. Hence, on the date set, the village-chiefs and their heads of collectivities appeared before this commission of war-chiefs, and were told that the king wished to know the number of women and girls belonging to the men who did not respect the order to go to war. It was stated that this information was desired so that a levy of these women and children might be made, for the men who had neglected the king's call were no longer desired, but that, in their place, these women were to be enrolled in the army. Now Dahomean tradition has it that in the earliest times women were warriors, but that a later king, Agadja by name, refused to accept women as soldiers because the Dahomeans, vanquished by the Yoruba people, had for a long time been held under tribute. However, in the reign of King Gezo, Dahomey once more prevailed over the traditional enemy, and it is from the time of this king, according to Dahomean accounts, that the battalions of Amazons date.

IV

In the palace of the king at Abomey were thirteen cases, each divided into two parts. They were kept in a room of the long, low house that has already been described, and were watched over by a woman who was charged with their care. It is quite possible, as was hinted by native informants, that the name of this house, Aido Hwedo, reveals the attitude of the king toward his population resources. For in the religion of Dahomey, Aido Hwedo is Dan, the serpent. In the belief of the Dahomean it is Dan who gives riches and who takes them

away,—"Dan is a thief," says the Dahomean, when speaking of him. Thus in the designation given this house may be reflected the extent to which the Dahomean monarch regarded his people as the ultimate source of his power.

In the Aido Hwedo house, then, rested these thirteen boxes. When a birth occurred during the year, a stone was put in the proper part of the first box, according to whether the baby was a boy or a girl. At the end of the year the woman in charge took the stones out of the thirteenth box and threw them away, for after a child had reached fourteen years of age, he was regarded as an adult and was included in the annual counting. The stones in the twelfth box were put into the thirteenth, those in the eleventh into the twelfth, those in the tenth into the eleventh, and so on until the second had been emptied into the third, the first into the second, and this last one, now empty, was ready to receive the stones which indicated the births for the new year.

Inside the palace walls there was another house named Abodji, watched over by a woman called Ndana. In this house, also, there were cases, but here they numbered fifteen. These cases received the stones which marked the deaths of Dahomean citizens. When a chief came to report deaths, he was asked the age and sex of the persons who had died. These reports were brought at short intervals during the year to the proper officials. For the region about Abomey, they were told to the Mingan, and for the other districts of the Kingdom to the chiefs who ruled over them. When a report was made at the palace, if the death was that of an adult male, a stone was placed in the fourteenth box; if it was a woman who had died, one was put in the fifteenth. If a boy aged three had died, a stone was placed in the proper half of the third case.

The question as to the manner in which a record of the age of these children was kept by a people who had no writing, poses itself here. Up to thirteen years, however, it was not difficult. Elders who remembered the birth of the child, could always be found; and since there was a "conquest" each year, the parent or relative of a child would merely count back yearly, campaign by campaign, until that of the year in which the child was born was reached, whereupon his proper age was stated. The count might be continued after a person was thirteen, but, as far as the monarchy was concerned, it was not obligatory to do so, since after this age the boy was legally a grown person. However, this manner of indicating the time of birth is still held to by older Dahomeans,

especially in the case of those who were born before the conquest of the kingdom by the French.

V

We may now draw these threads together. It will be remembered that the men of adult age, and the grown women who belonged to the men in the army, had been accounted for. Information as to the numbers of females over thirteen belonging to the men who did not go to war had been given the military commission, and for these, as for the foregoing, there were pebbles which corresponded in number to the number of persons involved. Children of both sexes up to the age of thirteen, had also been accounted for, and stones representing them were in the palace. In other boxes, finally, were represented the deaths for the year.

Now the two officials concerned with the statistics of the army were called upon to tell the number of men who had died on the field of battle. Two other officials, charged with the duty of informing the king as to the number of slaves and captives to be found in Abomey during the current year, also reported. A fifth official, to whom was given the duty of keeping count of the number of persons who had died of illness during the year, told his count. With these data in hand the final tally was ready to be made. The thirteen boxes which contained the pebbles representing the births of children were compared with the thirteen boxes which held the count of the dead, and the number of dead was subtracted from the number of the living. The number of men who died on the field of battle was subtracted from the tally of adult males given by the chiefs. The number of women who had died was added to the total of those living, and the whole number compared, as a check, with the net number of women the year preceding. The slaves, who were not counted with the Dahomeans, but had been kept as a separate tally, were added to the net numbers of Dahomeans. To these numbers the king added those taken captive in the campaign just ended; the pebbles for the males were put in the box for men, those of the women captives added to the box for grown women, and those of the children added to the stones of the proper sex and year, though in the case of captured children only approximate ages, of course, were available.[5]

[5] As an illustration of the care taken to keep the population from suffering in number through war-losses, the following may be taken: When the report came

The total was now at hand, and this gave the king information as to how many men, how many women, and how many children of each age up to thirteen were included in the population of the district of Abomey. At the same time the same process was being carried out at Whydah, at Allada, at Zagnanado, at Atakpame, at Save and Savalou, and in the region of Adja to the west. However, no net count was made by these chiefs, but all the boxes were carried directly to the palace at Abomey. That no mistake might be made there, the boxes were marked with one stripe for the one-year old children, two stripes for the two-year olds, and so on with fourteen stripes on the box containing the stones representing the adult males, and fifteen on that holding the count of adult females. Each chief went alone to present to the king his report and, as has been said, to repeat any figures about his particular district, would mean death.

When all the boxes were assembled, and the net count for all districts made, the total population of the kingdom of Dahomey for the year was known.[6] The stones, still in the small bags marked with the identifying devices of the separate villages, were placed in four large sacks. These were made of native raphia-cloth. One contained the pebbles which told the count of the men, a second those which represented the women, a third, the boys, and a fourth, the girls. In addition, there were three other sacks. One was black, and in this were put the stones that represented the men who had died in battle. Another was red, and here were placed the stones of those who had died of illness. In a final white one were placed the stones which showed

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of losses in battle, the captives were counted. Let us say four thousand Dahomeans had been killed, and six thousand captives taken. Then four thousand of these latter were retained to replace the man-power of the slain Dahomeans, being distributed throughout the kingdom to work the royal farms. The balance was divided into three parts; one part was kept by the king himself (these were mainly women, who were taken as concubines or household slaves); one part was sold as slaves; and the third part was distributed among the warriors and chiefs as a reward for valorous deeds.

[6] This is obviously idealised, given, as it was, almost forty years after the conquest. I am certain that the system did not operate, in the case of the out-lying provinces of the kingdom, with the smoothness here indicated, though I am convinced that it was effective in the vicinity of Abomey. The fact that it may have broken down in certain regions, however, does not vitiate the importance of the fact that the machinery for counting the entire population was provided in the organisation of the government.

the number of captives.[1] And to distinguish the four sacks in which were contained the pebbles, which told the number of living Dahomeans during that year, on the sack of the adult males there was sewed a small replica of the short trunks worn by every Dahomean man; on that of the women, some beads; on that of the boys, a representation of the male sex-organ; and on that of the girls, a small human figure with female sex-organs.

During the ensuing year, every eight days, there was sent to the king a wand covered with white cloth accompanied by sacks of pebbles. These showed the births of males and females in the district of Abomey. With these went another wand covered with black cloth, accompanied by other sacks, which showed the deaths of adult men and women and children of the respective ages and sexes. Every three months, a similar report was made to the king by each chief who ruled a district outside of Abomey. After the king had seen the sacks, the stones were placed in the proper boxes in the palace, so that at the end of the year, when the process that has been described was again gone through, there would be a control over the figures. But, most important, the king, in this way, could know whether the population was increasing or decreasing; he could know at any time how many people there were in his kingdom, and, if the number became smaller, he could summon his priests and soothsayers, to find the cause, and a "remedy."

The stones of the preceding year, in their sacks, were put in a new subdivision of the building called Aido Hwedo, which represented the true wealth of Dahomey, its population, there to remain as a permanent record. Only the king and the two officials who were charged with the care of the building might enter it. And all this was so that at the death of this king, these two might take the successor and say, "Young man, all your life you have heard Dahomey, Dahomey, but you have never until today seen the true Dahomey. For Dahomey is its people, and they are here."

[1] Just who these represented, is not clear, for this statement conflicts with the earlier one that the stones for the captives were added to those representing the Dahomeans. It may be that these represented the year's captives alone, and thus showed how successful the campaign had been, though this is only speculation.

THE RELATIONSHIP OF ERECT BODY LENGTH TO SUPINE BODY LENGTH[1]

BY CARROLL E. PALMER[2]

Throughout the history of anthropology workers in this line have given considerable attention to the necessity of employing standard procedures and nomenclature in anthropometry. In practice, however, it is often impossible or exceedingly difficult to maintain completely uniform standards during the amassing of extensive and diversified data. As a result, numerous studies have been undertaken to evaluate quantitatively the effects of varying certain factors and conditions. Reference may be made to the work of Scammon and Calkins (1929), who have estimated the effect of preservative solutions upon the linear dimensions of the fetal body, and to the work of Backman (1924), who has studied the physiological variation of body length during the day. In connection with this development it is the purpose of this paper to present a quantitative analysis of the difference between body length as measured in the erect and in the supine positions. The need of a method for bringing these two measurements into uniformity is apparent. Length of the infant and young child cannot be measured with the body placed in the erect position but, principally for practical reasons (Wilder, 1920), older children and adults are generally measured in the erect position. The construction of either individual or averaged growth curves, which include the period of infancy, thus makes it imperative to choose at the outset either one of two methods, namely, first, measure all individuals in the supine position, or second, evaluate a corrective factor for converting erect lengths into supine length

[1] Paper No. 163 from the Department of Biostatistics, School of Hygiene and Public Health, The Johns Hopkins University, Baltimore, Maryland.

[2] The data upon which this paper is based were collected during the writer's tenure as Fellow for Research in Child Development of the Laura Spelman Fund.

equivalents.[3] This paper proposes to deal with the latter method and will be limited to the postnatal growth period.

There is not general agreement at present among workers in the field regarding the names applied to the body length dimension. Length determined in the erect position is termed variously in the literature, *stature* (e. g., Hrdlička, 1920), *standing height* (e. g., Gray, 1922), *height* (e. g., Schwartz *et al.*, 1928 and 1931), *height standing* (e. g., Wilson *et al.*, 1930), and so on. Length determined in the supine position is termed *crown-heel length* (e. g., Scammon and Calkins, 1929), *recumbent length* (e. g., Hejinian and Hatt, 1929), *length* (e. g., Boyd, 1929), *height* (e. g., Baldwin, 1921), *lying length* (e. g., Schwartz *et al.*, 1928 and 1931), and so on. As far as is known the terms *erect length* and *supine length* have not been employed previously. However, these terms seem to possess certain obvious advantages and will be used throughout the paper.

General statements of the magnitude of the difference between erect and supine length are numerous in anthropological literature (e. g., Merkel, 1881; Baldwin, 1921; Gray, 1922). Growth curves of individual children between the ages of two and five years (Boyd, 1929) show that supine length is from one to one and one-half centimeters greater than erect length. Percentile tables (Wilson *et al.*, 1930) of height standing and of recumbent length for preschool children also show that recumbent length is one to one and one-half centimeters greater than height standing. Schwartz *et al.* (1928 and 1931) attempting to evaluate the reliability of measures of various biological characteristics of 50 adolescent boys published, among other things, correlation tables of standing height and length lying that show roughly that young boys are nearly one centimeter longer when lying than when standing.

[3] It must be understood in this connection that extrapolation by any method to obtain theoretical values of erect length of children not yet able to stand involves the postulation of the undefined phenomenon of a young infant's standing upright. In view of the difficulty of obtaining a clear understanding of this experimentally, it is hardly justifiable to employ theoretical erect lengths of infants. For this reason, it is necessary to use only supine lengths in any study which includes the whole of the postnatal growth period. And supine length must be measured directly in the case of infants while with older individuals it may be gotten either directly or by conversion from erect length measurements by means of formulas.

THE AUTHOR'S DATA

The material basis of the present paper consists of the two length measurements of 1123 normal, healthy individuals, 571 males and 552 females. The ages of these individuals ranged from 18 months to 20 years, with the number in each yearly age interval varying from 30 to 99 persons. Approximately 1000 members of the group were residents of the home for children maintained by the Royal Order of the Moose at Mooseheart, Illinois.[4] All of these children were measured by one observer, the writer. One hundred pairs of measurements were made upon pupils of the nursery school and kindergarten of the Institute of Child Welfare at the University of Minnesota.[5] All were measured nude except the girls over ten years of age, who were clothed in lightweight cotton bathing suits.

Erect length was determined according to the instructions for measuring *stature* outlined by Hrdlička (1920, page 66). The anthropometer used was incorporated into an especially constructed vertical upright. A small boxed platform attached to the base of the upright facilitated the uniform placing of the feet during measurement. As the subject stood upon the platform his heel, buttocks, shoulders and occiput were made to touch the vertical upright. Both visual and binauricular axes were made to take a horizontal plane. The anthropometer was graduated in the English scale and measurements were made to the nearest one-eighth inch. A single measurement only was recorded. The posture of the body during the determination of supine length was made to correspond in detail with the posture assumed during the determination of erect length. Measurements were made upon a horizontal plane surface to which was attached a vertical footboard and an anthropometer with a metric scale. This length was recorded to the nearest millimeter and a single measurement only was made.

Figures 1 and 2, for males and females, respectively, are scatter-diagrams of the two length measurements. In the diagrams the vertical axis *E*, represents length in centimeters measured in the erect position,

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[4] The writer wishes to express his appreciation to Superintendent E. N. Roselle and Dr. J. D. Nichols, Director of Health of Mooseheart, through whose interest and coöperation the collection of these data was made possible.

[5] These observations were made and generously contributed for use in this study by Dr. Edith Boyd of the Institute of Child Welfare of the University of Minnesota.

TABLE 1

Equivalent Values of Supine Length and Erect Length, Calculated from the Equation $E_m = -11.252 + 1.1490\,S_m - 0.000508\,S_m^2$

MALES

SUPINE *cm.*	ERECT *cm.*	SUPINE *cm.*	ERECT *cm.*
80	77.42	130	129.54
81	78.49	131	130.56
82	79.56	132	131.58
83	80.62	133	132.59
84	81.68	134	133.60
85	82.75	135	134.62
86	83.81	136	135.63
87	84.87	137	136.64
88	85.93	138	137.65
89	86.99	139	138.66
90	88.05	140	139.66
91	89.11	141	140.67
92	90.16	142	141.68
93	91.22	143	142.68
94	92.27	144	143.68
95	93.32	145	144.69
96	94.38	146	145.69
97	95.43	147	146.69
98	96.48	148	147.69
99	97.53	149	148.69
100	98.58	150	149.68
101	99.62	151	150.68
102	100.67	152	151.67
103	101.71	153	152.67
104	102.76	154	153.66
105	103.80	155	154.65
106	104.84	156	155.64
107	105.88	157	156.63
108	106.92	158	157.62
109	107.96	159	158.61
110	109.00	160	159.60
111	110.04	161	160.59
112	111.07	162	161.57
113	112.11	163	162.55
114	113.14	164	163.54
115	114.17	165	164.52
116	115.21	166	165.51
117	116.24	167	166.48
118	117.27	168	167.46
119	118.29	169	168.44
120	119.32	170	169.41
121	120.35	171	170.39
122	121.37	172	171.37
123	122.40	173	172.34
124	123.42	174	173.31
125	124.45	175	174.28
126	125.47	176	175.26
127	126.49	177	176.22
128	127.51	178	177.19
129	128.53	179	178.16
		180	179.13

TABLE 2

Equivalent Values of Supine Length and Erect Length, Calculated from the Equation $E_f = -8.994 + 1.1233\,S_f - 0.000464\,S_f^2$

FEMALES

SUPINE *cm.*	ERECT *cm.*	SUPINE *cm.*	ERECT *cm.*
80	77.90	130	129.19
81	78.95	131	130.19
82	80.00	132	131.20
83	81.04	133	132.20
84	82.09	134	133.20
85	83.13	135	134.19
86	84.18	136	135.19
87	85.22	137	136.19
88	86.26	138	137.18
89	87.30	139	138.18
90	88.34	140	139.17
91	89.38	141	140.16
92	90.42	142	141.16
93	91.46	143	142.15
94	92.50	144	143.14
95	93.53	145	144.13
96	94.57	146	145.12
97	95.60	147	146.10
98	96.63	148	147.09
99	97.66	149	148.07
100	98.70	150	149.06
101	99.73	151	150.04
102	100.75	152	151.03
103	101.78	153	152.01
104	102.81	154	152.99
105	103.84	155	153.97
106	104.86	156	154.95
107	105.89	157	155.92
108	106.91	158	156.90
109	107.93	159	157.88
110	108.95	160	158.85
111	109.97	161	159.83
112	110.99	162	160.80
113	112.01	163	161.77
114	113.03	164	162.74
115	114.05	165	163.72
116	115.06	166	164.69
117	116.08	167	165.65
118	117.09	168	166.62
119	118.11	169	167.59
120	119.12	170	168.55
121	120.13	171	169.52
122	121.14	172	170.48
123	122.15	173	171.45
124	123.16	174	172.41
125	124.17	175	173.37
126	125.17	176	174.33
127	126.18	177	175.29
128	127.19	178	176.25
129	128.19	179	177.21
		180	178.16

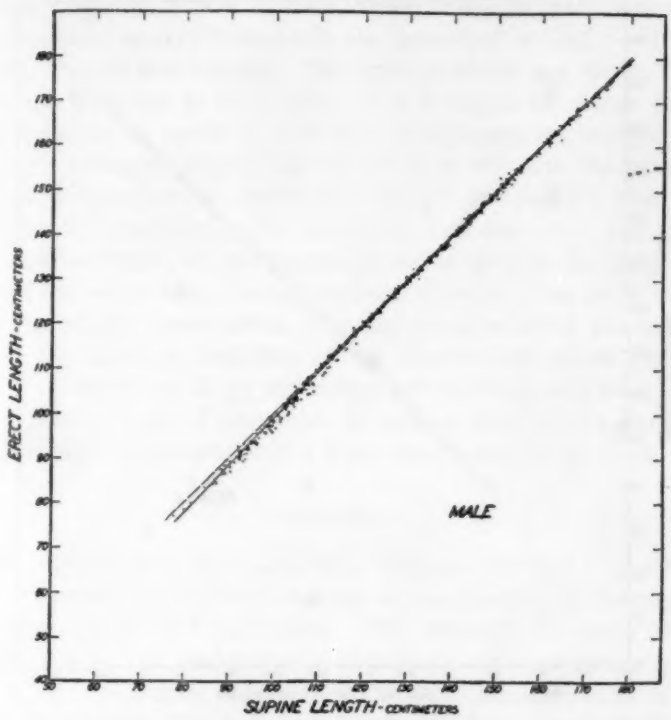

FIG. 1. SCATTER-DIAGRAM OF SUPINE AND ERECT LENGTH FOR 571 MALES

The solid line represents graphically the curve,

$$E_m = -11.252 + 1.1490\,S_m - 0.000508\,S_m^2.$$

The broken line represents graphically the curve, $E_m = S_m$. The unit is the centimeter.

and the horizontal axis S, represents length in centimeters measured in the supine position.

Two parabolic curves [6] were fitted by the method of least squares to the individual observations, one curve to the male data and the other

[6] Straight lines were fitted as the first step of the analysis and, although the relationship is not linear, certain points of practical importance may be derived from a consideration of the statistics of these lines. It was found that the slope and intercept constants of the equations representing the male observations were significantly different, in terms of sampling variability, from corresponding constants of the equations representing the female observations. It was concluded, therefore, that the data of each sex must receive separate study. Considering,

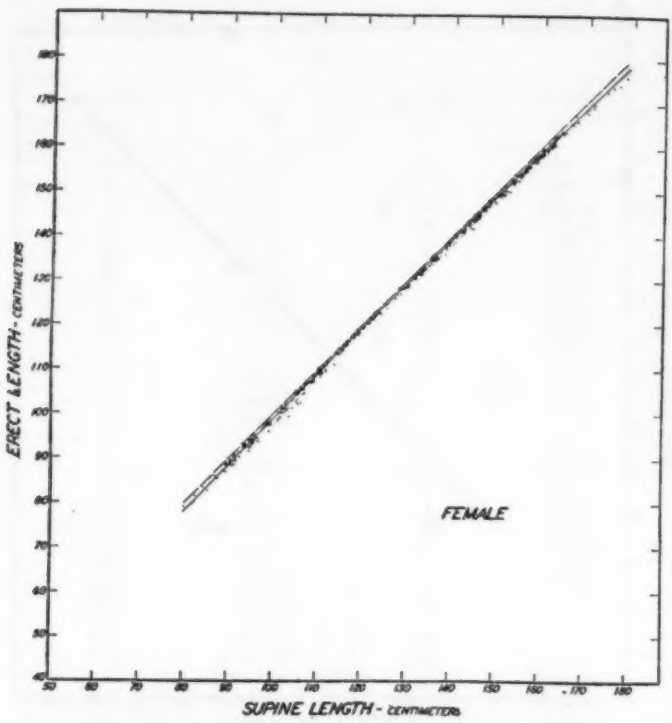

FIG. 2. SCATTER-DIAGRAM OF SUPINE AND ERECT LENGTH FOR 552 FEMALES
The solid line represents graphically the curve,

$$E_f = -8.994 + 1.1233\,S_f - 0.000464\,S_f^2.$$

The broken line represents graphically the curve, $E_f = S_f$. The unit is the centimeter.

to the female data. The end result may be written in the form of two equations, thus:

(1) For males: $E_m = -11.252 + 1.1490\,S_m - 0.000508\,S_m^2$

(2) For females: $E_f = -8.994 + 1.1233\,S_f - 0.000464\,S_f^2$

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then, the male and female data separately, it was found that the equation obtained by minimizing the vertical deviations from the line was essentially identical (when account is taken of sampling variability) with the equation obtained by minimizing the horizontal deviations. Thus, for the practical purposes of this study, it is quite immaterial whether vertical or horizontal deviations are minimized for the fitting of the curvilinear equation. The final analysis consisted, therefore, of the fitting of a second degree parabola to the observations of each sex separately and in both cases the deviations of supine length from the curve were minimized.

As in the diagrams, E and S refer, respectively, to erect and supine length measured in centimeters and the subscripts m and f refer, respectively, to male and female. The fitted parabolas are shown graphically in the diagrams as solid lines. The broken lines shown slightly above the cluster of points in both figures represent the straight lines, erect length = supine length. At any point on this line, the horizontal or vertical distance between the broken straight line and the fitted parabola represents graphically the difference between erect and supine length, and inspection of the figures shows in general the progressive change of the relationship during postnatal growth. The error of estimating one length measurement from the given value of the other, in terms of the standard deviation of the observations about the fitted curves, is of the order of .75 centimeter and although this error is less than the total amount of correction in certain parts of the curve, the correction itself is systematic and therefore cannot be ignored.

DISCUSSION

A sex difference in the relationship between erect and supine length was not expected *a priori,* but is evident on examination of the diagrams and by analysis of the fitted curves. The essential difference between the sexes lies in the tendency of erect length to approximate supine length in the adolescent males. No general rationalization will be attempted to explain this variation, but it will be pointed out that the discrepancy may be due to differences in coöperation during the process of measurement. Girls between the ages of twelve and twenty, and particularly the older girls, were often shy, and indifferent during the measuring. The boys of this age, on the other hand, were always actively interested and not a little proud of their physical status. There is, at least, a suggestion that when these boys were asked to stand for measurement they stood, as a group, slightly "taller" than the girls and that this factor did not operate to the same extent in measurements in the supine position. Although no specific evidence can be brought forward to substantiate this hypothesis, the writer is of the opinion that such factors can bring about the observed sex differences. Whatever factors were in operation, however, were undoubtedly significant and might occur quite generally in measurements on American children.

The utility of the fitted curves as conversion formulas has been enhanced by the construction of Tables 1 and 2, which provide for each sex the calculated value of erect length equivalent to a given value of

supine length. Conversion values are recorded for each centimeter in the interval from 80 to 180 centimeters. This range is sufficient for most anthropometric needs and involves extrapolation at both ends of the scale as far as seems justifiable on the basis of the observations available.

SUMMARY

1. Measurements of total body length in both supine and erect positions were made of 571 males and 552 females between the ages of 18 months and 20 years.

2. Parabolic curves, fitted by the method of least squares to the male and female observations, were determined as follows:
For males, Erect length =

$$-11.252 + 1.1490 \text{ supine length} - 0.000508 \text{ (supine length)}^2$$

For females, Erect length =

$$-8.994 + 1.1233 \text{ supine length} - 0.000464 \text{ (supine length)}^2$$

3. Tables are presented which record for each sex the calculated value of erect length equivalent to a given value of supine length for each centimeter in the interval from 80 to 180 centimeters.

REFERENCES CITED

BACKMAN, G. 1924. Longueur du corps au cours de la journée. *C. R. Soc. Biol. Paris*, 90; 1118-1120.

———. 1924. Körperlänge und Tageszeit. *Upsala Läkareförenings Förhandl.*, N. F., 29; 255-282.

BALDWIN, B. T. 1921. The physical growth of children from birth to maturity. *University of Iowa Studies. Studies in Child Welfare*, I, *Iowa City*, p. 35.

BOYD, EDITH. 1929. The experimental error inherent in measuring the growth of the human body. *Am. J. Phys. Anthrop.*, 13; 389-432.

GRAY, H. 1922. Sitting-height and stem-length in private school boys. *Am. J. Dis. Child.*, 23; 406-418.

HEJINIAN, LUSEA, and HATT, ELISE. 1929. The stem-length: recumbent-length ratio as an index of body type in young children. *Am. J. Phys. Anthrop.*, 13; 287-307.

HRDLIČKA, A. 1920. Anthropometry. The Wistar Institute. *Phila.*

MERKEL, R. 1881. Ueber tägliche Grössenschwankungen. *Verhl. d. Ges. Chirurgie*, 10th Cong. (*Berlin*), 125.

SCAMMON, R., and CALKINS, L. 1929. The Development and Growth of the External Dimensions of the Human Body in the Fetal Period. *Univ. Minn. Press.*

SCHWARTZ, L., BRITTEN, R., and THOMPSON, L. 1928. Studies in physical development and posture. I-III. *Public Health Bull.* 179, *Washington.*

——. 1931. Studies in physical development and posture. IV. *Public Health Bull.* 199, *Washington.*

WILDER, H. H. 1920. A Laboratory Manual of Anthropometry. Phila.

WILSON, C. A., SWEENY, MARY E., STUTSMAN, RACHEL, CHESIRE, LEONE E., and HATT, ELISE. 1930. The Merrill-Palmer Standards of Physical and Mental Growth. The Merrill-Palmer School, *Detroit.*

SOME FACTORS RELATED TO DIFFERENTIAL SEX-RATIOS AT BIRTH

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The proportion of males born to females born is a phenomenon apparently purely physiological in scope. There are rather interesting clues, however, which point to possible *social* factors as variables which are related to this phenomenon in such a way as to affect the numerical proportion of the sexes.

The sex-ratio at birth is measured in terms of the proportion of males born alive to females born alive. In the United States, the sex-ratio at birth is usually regarded as being 105, i. e., 105 males are born alive per 100 female live births.

Sex is apparently determined at the moment of fertilization. The present paper proceeds on the basis of this theory. The hypothesis which is to be tested here is that social factors affect the sex-ratio at the time of birth. Sex itself is unchangeable in pre-natal existence.[1]

Wherein, then, does the possibility of altering the sex-ratio at birth lie? It must be remembered that sex is physiologically determined at conception, and the sex ratio discussed is that which prevails at birth. Any alteration in the proportion of the sexes must therefore occur during the approximately nine months' period of intrauterine existence. As is well known, there is a heavy mortality in connection with the actual birth of children. The proposition may be made, therefore, that if one is to find clues to possible factors affecting the sex-ratio at birth, one must seek them, first, in the period between conception and so-called birth, and second, in the factors which produce the heavy mortality of fetuses prior to birth.

[1] It may be noted that sex reversal in man takes place, if at all, in a negligible number of cases, and this phenomenon may be disregarded for all practical purposes.

Nor would these be enough to prove the theory under consideration. Since the present paper deals with social factors contributing to a *high* sex-ratio at birth, it undertakes to show that a high sex-ratio at conception becomes lowered through a differentially higher male than female mortality. In connection with this hypothesis, it must be demonstrated that social factors are contributing agencies in this phenomenon.

THE SEX-RATIO OF STILLBIRTHS

Specifically, one may first turn to the data on pre-natal deaths. According to American statistics for the Registration Area, there are approximately 4 stillbirths per every 100 live births. As deaths go, this is a high ratio, and in the nature of the case, an important percentage of stillbirths are never reported. (Incidentally, it may be noted here that the practice of the Census Bureau is to include in the term "stillbirth" all deaths prior to or at birth, including not only stillbirths but also abortions.) The probabilities are that the actual percentage of pre-natal deaths is considerably higher than the figures show. Obviously there is a high pre-natal mortality.

The next step is the consideration of the evidence for a differential pre-natal mortality which, to meet the tests of the hypothesis, must bear more heavily upon males than upon females. In 1918, and regularly since 1922, the United States Bureau of the Census has collected stillbirth data for the Registration Area. In the data available through 1928, there were over 570,000 stillbirths for the Registration Area alone. The computed sex-ratio for these stillbirths reaches the astonishing proportion of 135.6 males to 100 females. This result is corroborated by the data analyzed by other investigators for the various European countries.[2] There is a high mortality during the pre-natal existence of infants, and this mortality operates more heavily upon males.

The next point to consider is whether this differential mortality, operating against the male sex, operates still more heavily as one approaches the point of conception. If this is found to be the case, it may be considered as indirect but positive evidence that there is an exceedingly high, normal sex-ratio at the point of fertilization. Data are also available on this point. The United States Bureau of the Census has

[2] For references to the literature on this point, consult an article by the writer, "The Influence of Social Factors upon the Sex-Ratio at Birth," *American Journal of Sociology*, XXXVII (1931), 3-5.

obtained, from certain selected districts of the Registration Area, a census of deaths, by sex, for each month of uterogestation, since 1922. The results of computations based upon the data collected for the years 1922 to 1928 inclusive are as follows:

TABLE 1

Sex-Ratios of Stillbirths According to Period of Uterogestation, Selected Areas in the United States Registration Area, 1922-1928[3]

PERIOD OF UTEROGESTATION	NUMBER OF MALE STILLBIRTHS	NUMBER OF FEMALE STILLBIRTHS	SEX-RATIO OF STILLBIRTHS
Under 4 months	1,425	386	369.2
4 months	2,717	1,290	210.6
5 months	4,815	3,320	145.0
6 months	6,665	5,282	126.2
7 months	5,471	4,612	118.6
8 months	8,639	7,088	121.9
9 months and over	31,836	23,056	138.1

In this table, the earliest category is "Under four months." The data for these early months are as yet too few to compute separately. The analysis of the figures in Table 1 brings out interesting results. Beginning with the period nearest conception, the sex-ratio declines steadily with each month until the period of birth approaches, when it rises rapidly. The conclusion here seems inevitable. A high mortality of males accompanies the last months of pre-natal existence due to the difficulties brought about by the strain of birth. Aside from this, starting with the seventh month of intrauterine existence and proceeding backwards toward the earliest months, the sex-ratio of mortalities rises rapidly from a ratio of 118.6 in the seventh month to 369.2 during the period preceding the fourth month of existence.

Proceeding backwards, then, from the sex-ratio at birth through the progressively higher sex-ratios of intrauterine mortality, there is evidence of a much higher sex-ratio at conception than at birth. There have been various scientific estimates of the probable sex-ratio at conception. Dr. Adolph Schultz estimates a sex-ratio at conception of

[3] Computed from data obtained from Bureau of the Census, *Birth, Stillbirth, and Infant Mortality Statistics* (1922), p. 40; (1923), p. 26; (1924), p. 31; (1925), Part II, p. 42; (1926), p. 44; (1927), p. 45; (1928), p. 235.

108.47,[4] a decidedly conservative figure. Other calculations, derived by European investigators, range from 108.7 to 116.4, although Auerbach, on the basis of extensive European data, adds that if corrections were made, the truer result would be somewhat more than 125 males produced per 100 females.[5] The analysis of the American data in conjunction with the quoted results leads to the conclusion that there is a primary, i. e., at conception, sex-ratio of at least 110 males per 100 females. In the very nature of the case, a more precise figure is not warranted. Whatever error exists lies on the safer side of a too low estimate, due to the fact that the percentage of reported cases is lower for the earlier than for the later months, and, as has been noted, it is during the earliest months that the ratio of male mortality is highest.

The conclusions reached thus far may be summed up as follows: *First,* there is a high sex-ratio at conception which may be conservatively estimated at 110 males per 100 females. *Second,* at birth the sex-ratio has become lowered to 105 males per 100 females. *Third,* the difference between the two sex-ratios is biologically and mathematically accounted for by the differential pre-natal mortality rates which affect males more severely than females. It follows from these conclusions that the sex-ratio at birth is a function of the sex-ratio at conception and the sex-ratio of pre-natal mortality.

SOCIAL FACTORS AFFECTING THE SEX-RATIO

In solving the problem of possible *social* factors affecting the sex-ratio at birth, there are certain factors which operate to produce an unfavorable environment for motherhood and thus directly affect the mother's ability to provide the conditions essential to the development of the fetus. Failure to guard the health of the mother is dangerous not only to the mother but also to the child. An enormous mass of medical literature attests to the fact that in addition to infections, unfavorable toxic conditions due to such variables as undernutrition, overstrain, excessive fatigue and nerve depletion, to mention merely the less debatable factors, all offer serious pre-natal hazards. Mothers in poorer economic and social positions, in modern complex societies, must usually carry on daily activities in the home and in the shop and factory,

[4] "Studies in the Sex-Ratio of Man," *Biological Bulletin,* XXXIV (1918), 264-5.

[5] "Das wahre Geschlechtsverhältnis des Menschen," *Archiv für Rassen- und Gesellschaftsbiologie,* IX (1912), 10-17

which, in many cases, are deleterious to the health not only of the mother, but also of the child, and may even eventuate in the death of the infant. As has been noted, the pre-natal deaths bear more heavily on males than upon females. From government data for selected areas of the United States, the sex-ratios were computed for over 5,000 stillbirth cases, for the years 1922-1928 inclusive, where such deaths were listed under the general classification of "traumatism and overwork."[6] For these cases, the sex-ratio of mortality was 147.8. The deaths under consideration here were practically all caused by some deleterious environmental factor which directly or indirectly affected the unborn child. For the 341 cases coming under the definite rubric of "overwork," the sex-ratio of mortalities was 150.7.

Nor is overwork, largely due to economic conditions, the only factor. Another important factor operating to increase the number of pre-natal deaths is what may best be termed ignorance. Practitioners among the poorer and less educated whites and Negroes are at one in ascribing an important part of pre-natal mortality to simple ignorance, including reliance upon hearsay and superstition, in regard to what should be done in order to assure the welfare of the child. There are no actual data available as to the sex-ratio of mortalities associated with the factor of ignorance. Knowledge of the differential nature of intrauterine mortality, however, warrants the definite assumption that there would be both a lower pre-natal mortality and a higher proportion of males born alive were ignorance overcome.

Socially, economically, and educationally lower classes may be expected to have a comparatively high proportion of pre-natal mortality. This mortality would operate more heavily against male fetuses than against female fetuses. These factors would in turn operate to reduce the potentially high sex-ratio at birth to one considerably lower. The converse of this is that socially, economically, and educationally "higher" classes, having the capacity of providing more desirable environments for motherhood, should assure a lower pre-natal mortality, which in turn would operate in favor of a higher sex-ratio at birth than the average sex-ratio of 105 + males per 100 females.

A socially select group, meeting the necessary qualifications, would furnish the opportunity for analyzing the influence exerted by such favorable conditions. Such a group is available. Genealogical records

[6] *Birth, Stillbirth, and Infant Mortality Statistics* (1922), p. 302; (1923), p. 203; (1924), p. 227; (1925), p. 184; (1926), p. 190; (1927), p. 184; (1928), p. 237.

exist which contain data pertinent to testing the general hypothesis. The data which follow were obtained from records of descendants of early American families.[7] The families listed have the following qualities necessary for the present purpose. They are relatively homogeneous. They form a highly selected group on the basis of economic, social, and educational criteria. A further element of selectivity lies in the fact that on the whole the best known families have been culled from the even larger group of descendants of early Americans. The present generation only was utilized with resulting greater accuracy of the data.[8]

From the data available, 5,466 families of the present generation could be utilized. It was necessary that these families be regarded as already completed, i. e., that no more children could be expected. The criteria for completed families were, (1) death of either parent, (2) divorce, (3) the attainment by the wife of the age of forty-five years, or (4) childlessness for at least nine years.

In these 5,466 families, 15,763 children were born alive. Eight thousand, three hundred, twenty-nine of these were males; 7,434 were females. The sex-ratio at birth for these children of higher class families was 112.0. This compares with the sex-ratio for the country as a whole of 105 + males per 100 females. This group provides higher than average types of training and resources for the care of the mothers. In doing this it has automatically secured for itself an importantly higher proportion of male children than the average for society in general, which on the whole is less equipped to perform this function than the more favored group.

The data were then utilized to test the theory further. In a previous study,[9] correlations for size of family and various social factors were obtained and analyzed. One conclusion reached was that size of family decreased with improved social, economic, and educational conditions. Proceeding from this, the data utilized in the present study were separated and tabulated according to number of children per

[7] F. A. Virkus (ed.), *The Abridged Compendium of American Genealogy* (Chicago, 1925, 1926, 1928), Vols. I, II, and III.

[8] Since the great bulk of the names listed are for men, the preceding generation would furnish a spuriously high proportion of males. When their children only are taken, this source of error is eliminated.

[9] Sanford Winston, "The Relation of Certain Social Factors to Fertility," *American Journal of Sociology*, XXXV (1930), 753-64.

TABLE 2

Sex-Ratio at Birth According to Number of Children Born per Family[10]

NUMBER OF CHILDREN	NUMBER OF FAMILIES	NUMBER OF MALES BORN	NUMBER OF FEMALES BORN	SEX-RATIO
1	1,166	629	537	117.1
2	1,463	1,604	1,322	121.3
3	1,223	1,945	1,724	112.8
4	820	1,720	1,560	110.3
5 or more	794	2,431	2,291	106.2
Total	5,466	8,329	7,434	112.0

family. Interestingly enough, there were so few families of 5 or more children that all families having at least five children were grouped in the one category.

The very high sex-ratio for the one- and two-child families is apparent. It is considerably higher than the sex-ratio for the entire group, irrespective of family size. The three- and four-child families approach the average of the entire group, whereas for the grouping, 5 or more children, the sex-ratio drops to 106.2, a ratio only slightly higher than that of the general American population. The results show that as the size of the family increases, the "normal" sex-ratio is approached by the socially select group under consideration.

The general hypotheses thus far advanced lead to the expectancy of these results. It is believed that the significantly high average sex-ratio at birth for the whole group is related socially as well as mathematically to more favorable social conditions. This is probably particularly true with respect to the smaller families.

The conclusion that economic conditions are effective is borne out by a study made in London,[11] where, dividing the population analyzed

[10] The real significance of the variations in the percentages of males is seen best when the one- and two-child families are combined and when the three- and four-child families are combined. For the former the percentage of male births is $54.57 \pm .53$, and for the latter $52.74 \pm .40$. The percentage of males for five or more children is $51.48 \pm .49$. The difference between three- and four-child families and families of five or more is $1.26 \pm .64$. The difference between one- and two-child families and families of five or more is $3.09 \pm .72$.

[11] R. C. Punnett, "On Nutrition and Sex-Determination in Man," *Proceedings of the Cambridge Philosophical Society*, XII (1902-4), 262-76. See particularly page 276.

according to economic status, it was found that the proportion of males born was highest in the wealthiest group, intermediate in the middle groupings, and lowest in the poorest grouping, a result completely in accord with the hypothesis set forth in the present study. Indirectly there is corroboration of another sort. Available European data show an importantly higher stillbirth ratio among the poor than among the wealthy.[12] In another investigation, the stillbirth ratio was found to be over twice as high among laborers as among those forming the upper classes.[13] The connection between a high proportion of stillbirths and a lower percentage of males produced has been brought out previously in this discussion.

This investigation, therefore, leads to the conclusion that there is an important relationship existing between socially desirable factors and a higher ratio of males born alive to females born alive. The high sex-ratio at birth has been attributed to the fact that those classes which have the benefit of more nearly optimum environments have a tremendous advantage with regard to adequate provision for the unborn child. The greater the chances of survival for the fetus, the greater the proportion of males born alive will tend to be. Such being the case, it is only logical to state that when society as a whole provides a more adequate socio-economic environment, a higher sex-ratio at birth for the general population should eventuate. Certainly it is conservative to state that the social factors presented in this study are agencies contributing to this end.

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[12] A study based on data for Holland by V. Stuart. Quoted in A. Schultz, "Studies in the Sex-Ratio of Man," *Biological Bulletin*, XXXIV (1918), 269.

[13] *Ibid.*, p. 269. See data by Conrad for Halle.

NOTES

THE PSYCHO-GENETIC RELATIONSHIP

INTRODUCTION

In a recent paper [1] principles were set forth refuting the so-called behavioristic psychology. This treatment was presented from the background of a specialized branch of biology, genetics, only. It is the purpose of this paper to call attention to the fact that the controversy between nativism and empiricism is a pseudo-problem—the problem disappears when the true relationship between psychology and genetics is perceived.

The apparent key-note of the contribution cited above is radically nativistic. Those two authors reinforce their argument by the presentation of eight partial case studies, and they use methods of trait analysis of doubtful validity. Furthermore they erroneously claim John B. Watson and his followers to be extreme supporters of the traditional empiristic school.

It is indeed unfortunate that a gap should exist between the sciences of psychology and genetics. Such a breach is not conducive to a concise set of working hypotheses in either of the two fields, nor does it lead to a condition such as will bring about a significant coöperation on the part of workers in both branches of science. Human psychology is in reality a biological science. Both classes of workers have as their subject material a common substrate, protoplasm, in its highest form of differentiation.

THE GENETIC DATA

The paper in question deals with the concepts of mental age and special abilities, and the authors attempt the difficult task of these trait analyses. The data were collected by them through correspondence with the physician in charge of the patients, and by personal interview.

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[1] Rife, David C., and Snyder, Laurence H., Studies in human inheritance VI, A genetic refutation of the principles of "behavioristic" psychology, HUMAN BIOLOGY, Vol. 3, No. 4: pp. 547-559, Dec. 1931.

The first case considered was a low grade moron whose mother was regarded as mentally deficient. This boy had a special ability for drawing and curiously "readily drew excellent pictures of such objects as he could understand."

The second case, one which we shall consider typical of the idio-savant type, was a 27 year old male with a mental age of three years, who was blessed with a special mathematical ability. He, although an idiot, could continue a geometrical series of figures. He could also work square root, and multiply mentally. This boy's father has a feeble-minded first cousin. None of the other relatives is known to be feeble-minded, while father and mother, and paternal grandparents, are represented as normal. To quote Rife and Snyder, "No outstanding mathematical ability is found among the family, although the mother is fond of and good at mental arithmetic."[2] This statement appears to cast doubt on the main thesis of the authors. The individual also had five filial relatives, all apparently normal and without special ability.

The subject in case three is called musically specialized, which ability is "expressed in the presence of *acquired feeble-mindedness.*"[3] Case four deals with a so-called low grade moron of special mechanical ability. Case five considers a male imbecile with a special artistic ability; of the 37 relatives who are known, only the mother was classed as mentally deficient, the father being normal, and the latter was said to possess drawing ability.

Case six is concerned with a female idiot of low grade; nothing is known concerning her parentage, but she is said to have musical ability, which is, it is supposed, a result of germ plasm inheritance. Yet two of her normal sisters and one normal brother have learned to play and sing. Is it not possible that, by training and conditioning, she could have *learned* to play and sing too? Again we note a rather pointed distinction between social and germ plasm inheritance.

Another analogous case is one in which nothing is known concerning the parents of an idiot boy who plays the piano by ear, and who has a normal sister who possesses marked musical ability. Moreover Rife and Snyder report several cases in which a phenomenal memory manifested itself in the presence of feeble-mindedness. "Although nothing could be learned of the family histories in these cases, *it is clear that no training was involved in the development of this ability.*"[4]

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[2] *Ibid.*, p. 554.
[3] *Ibid.*, See p. 555. Italics mine.
[4] *Op. Cit.*, p. 556. Italics mine.

The idio-savant is supposed to be specially endowed, and of inferior intelligence. The two fundamental concepts underlying the entire treatment are (1) ability, and (2) intelligence. These two terms simply cannot be scientifically defined. They refer to nothing concise and definite. They are social constructs, abstractions.

Let us consider now the idio-savant of case two, who could count. This is an example of a high type of symbol manipulation. It is true that thinking has been associated with intelligence. Our intelligence tests do not, however, test thinking as such. The ability to think is alone not a criterion of intelligence. One may be able to handle symbols, yet be far below the imaginary norm. On the other hand, one may be totally incapable of manipulating more than fractions, yet adapt himself to the group perfectly. In nature there are no definite units, no "faculties" or special abilities of the mind. There are an infinite number of so-called special abilities which must be cultivated or stimulated and which are synthesized in our concept of the hypothetical normal individual. We might draw an analogy from another class term—health. As many different states of parts, working as a whole, go to make up the supra-summative entity which we refer to as health, so the functioning of many different dynamic processes goes together to comprise our intelligence.

In addition to the doubtfulness of their trait analysis, Rife and Snyder could not possibly reach a valid *a posteriori* conclusion on so few cases. Furthermore the method of collecting data is obviously not trustworthy. The data are essentially psychological in nature, and must be treated as such if they are to throw light upon the geneticists' problem.

THE WATSONIAN PRINCIPLES

Let us see exactly what Watson himself has to say in regard to germ plasm inheritance, and how he disposes of the problem of the inheritance of special 'mental' talents and feeblemindedness, as this is the crux of the objection of Rife and Snyder in their 'refutation' of the principles of behaviorism. Watson's central thesis is indicated by the following quotation: "Man is an animal born with certain definite types of structure. Having that kind of structure he is forced to respond to stimuli at birth in certain ways. . . . . There exists a certain amount of variation in each. This group of man's reactions we shall call man's unlearned behavior." [5] As one well versed in genetics

[5] Watson, John B., Behaviorism, p. 74.

Watson is careful to emphasize that structure, not function, is inherited. To further bring out this standpoint, Watson states: "I wish to draw the conclusion that there is no such thing as an inheritance of *capacity, talent, temperament, mental constitution,* and *characteristics.* . . . . A certain type of structure, plus early training, accounts for adult performance." [6]

Such a statement absolutely contradicts the assertion of the above cited article by Rife and Snyder. Watson thus certainly has not slighted the genetic aspect of human nature. On the contrary he has keenly perceived the genetic relationship. The question arises,—is there not danger that the geneticist is neglecting (at least the paper of the two authors mentioned would tend to indicate such a danger) certain environmental forces such as social heritage and training factors, in stating that special abilities are hereditary?

In his understanding of the development of the individual organism Watson conceived of the individual as an integrated protoplasmic system in which thousands of variations are laid down in the germ plasm. The function is a resultant *process,* which is supra-summative. By information gleaned from psychological experimentation the behaviorist has merely pointed out again the conclusion reached by Child, that ". . . . it becomes quite clear that the order and unity of the organism are really products of the reaction of the protoplasm. In the broad sense, they are the products of behavior. The protoplasm from the first moment of its existence, is behaving with reference to its environment. In this general biological sense, life is reaction to environment." [7]

It is obvious that an infant possessing only half of the normal amount of neural connections, or possessing the potentiality of little or no maturation (myelination) would not be a normal specimen. Doubtless such a creature would become a feeble-minded person. Watson cautiously guarded his statement against just such a possibility. Yet Rife and Snyder used such cases to prove their point in their attempt to reflect skepticism upon the principles of the behaviorist.

The acquisition of these special abilities is a special case of the learning process. Just as the more overt responses show development and adaptation of the more gross muscles and structures of the body, so the symbol manipulation shows that adventitious changes have been

[6] *Ibid.,* p. 75.

[7] Child, C. M., The organism as a behavior pattern, *Bulletin of Society for Social Research,* No. 2, Aug., 1926.

wrought in the finer, more delicate muscles and structures of the body, particularly those which have to do with movements in the larynx, used in speech. The mathematician is a thinker because he uses symbols. But the term 'thinking' means nothing psychical or mentalistic to the behaviorist. It is behavior, the movement of the finer muscles of the body—sub-liminal, implicit speech. It is a peripheral voci-motor process for Watson, i. e. actual innervations of the vocal apparatus, the larynx and tongue. The events are, then, verbal, and are carried on as implicit language responses. The concept was not new with Watson, being presented by writers as early as H. A. Taine in 1870. The alleged special mathematical ability of case two cited above is learned, as is any other skill.

Thus one must not say that Watsonian behaviorism is unsound in its thesis that mental development is due to environmental factors. Development implies gradual adventitious change. The environmental forces, playing on the integrated protoplasmic substrate, direct these changes. These principles are fundamental in the behavioristic system. This system has governed a multitude of experiments. It means a biological approach to human personality. It has given an impetus to objective research and has been a forward step in the formulation of a concise set of working principles which are compatible in both the fields of psychology and genetics.

A SUGGESTED MODIFICATION

It is apparent that Watson regarded the products of the germ plasm as related to the environment, and that his main interest lay in an observation of the responses of the organism to its stimulating conditions. He has been criticized by the majority of psychologists, not on account of the inclusion of the interacting environmental forces, but because the genetic-environmental relationship was too static. Indeed the relationship itself is, of necessity, included in any psychological system.

Learning in our idio-savant type is not dissimilar to any other type of adventitious change. It is the result of many influences, the central one being the play of environmental change. The relationship of the individual, a genetic product in the unlearned state, with the environment, is a dynamic one. The individual must, to learn, possess a motive or appetite. This need itself is a result of the genetic and environmental forces interacting, e. g. tissue need, hunger. A second condition, one which was keenly perceived by the functional school of psycholo-

gists, is that of a problematic situation or block in the way of a smooth course of activity tending toward its solution, or the goal, the attainment of which brings about a condition of balance or resolution of the strained relationship between the individual and his environment.

A third condition for learning is a degree of maturation of the individual sufficient that a change in these relationships can occur, i. e. that the resolution of tension can be brought about. The fact that a tension does persist in some cases accounts for some forms of functional mal-adjustments.

The actual learning process consists in an evolution of perceptual relationships of the environmental factors in the resolution of tensions, or the reorganization of the field resulting in the removal of the obstruction to routine activity. In this continuous process the two forces are in constant dynamic interaction and are inseparable from one another. Always the environment plays its part and a changed environment means a change in the resulting product.

BIBLIOGRAPHY

1. Child, C. M. The organism as a behavior pattern. *Bulletin of Society for Social Research*, No. 2. 1926.
2. Jennings, H. S. Heredity and environment. *Sci. Monthly*, 19, 1924.
3. Koffka, K. The Growth of the Mind. *New York;* 1927.
4. Kuo, Z. Y. The fundamental error of the concept of purpose and the trial and error fallacy. *Psych. Review*, 35, 1928.
5. Pavlov, I. P. Conditioned Reflexes. *London*: 1927.
6. Peterson, Joseph. Learning when recency and frequency factors are negative. *Jour. Exp. Psych.*, 5, 1922.
7. Thorndike, E. L. Human Learning. *New York*: 1931.
8. Tredgold, A. F. Mental Deficiency. *New York*: 1922.
9. Watson, J. B. Behaviorism. *New York*: 1925.
10. ———. Psychology from the Standpoint of a Behaviorist. *Philadelphia*: 1919.

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QUOTATIONS

RESTRICTIONS ON MARRIAGE IN EIGHTEENTH CENTURY BAVARIA *

With the rise of the state to power during the Renaissance and later, marriage, which had been during the Middle Ages under the exclusive control of the Church, was subjected to regulation by the civil authorities. In some countries civil marriage was established as the only legally binding form; in others the religious ceremony was retained, but a license for the marriage from the civil authorities was required. Now the power to license is the power to prohibit. For the most part these civil restrictions referred to such matters as age, parental consent, and consanguinity. The Electors of Bavaria, however, in their zeal to solve the problem of pauperism out-Malthused Malthus by decreeing that no marriage license should be issued except to persons capable of supporting a family. The story, as told by Dormitzer in *Die administrativen Verehelichungsbeschränkungen in Bayern*,[1] is curious enough.

Since the creation of restrictions on marriage the execution of which rests with the police authorities was based on the idea that unrestricted marriage of the proletariat would result in an oppressive increase of the burden of poverty, most of these enactments are found in the poor laws, which had become very voluminous in the previous centuries, or in connection with the legal regulation of the condition of the poor and lower classes. The first prescriptions of this kind for Bavaria are found in Title XII, Book IV [of the Ordinance of 1616] against "frivolous marriages of domestics," i. e., against persons in the service of others. In these the clergy are admonished no longer to bless such "frivolous marriages"; "they must then show a certificate that the authorities permit them this, in which permission the authorities shall take the common weal, propagation, education, honor,

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[1] Die / administrativen Verehelichungsbeschränkungen / in Bayern / bearbeitet und der hohen Juristenfakultät der Universität Erlangen behufs / Erwerbung des Doktorgrades vorgelegt / von / Sigmund Dormitzer / Rechtspraktikant / in / Nürnberg. / Druck von Fr. Monninger in Nürnberg. [n. d.] Pp. 59. 14.3 × 21.9 cm. I wish to thank Miss Hermine Grimm for checking my translation of this passage.

and respectability into due consideration, and not concede too much to the young people herein, nor give them the aforesaid certificate without weighty cause." Persons who in spite of this regulation have married without permission from the authorities, shall nowhere be taken in or sheltered. (pp. 5-6)

While the Ordinance of 1616 sets up restrictions on marriage only for domestic servants and the poor, the similar provisions of the Beggary Edict, although they too, being essentially poor laws, have primarily only undomiciled persons in mind, apply nevertheless to all subjects of the state indiscriminately, in that permission from the authorities was necessary for all. The Beggary Edict of July 27, 1770—that of March 3, 1780 is essentially the same as the earlier edict—decrees in Article 10 that "married but undomiciled persons shall betake themselves to the place where they were married with the proper permission of the secular authorities." Then the edict admonishes the bishops of the Electorate that they shall allow no subject to be married "without the exhibition of Our express license or that of the civil authorities appointed by Us," and the express command is given to the magistrates that, if a marriage has been contracted without the production of such license, the parties concerned shall be banished from the Electorate and, if they return, shall be dealt with like alien vagabonds.

The clergy, however, who marry anyone in contravention of the aforesaid decree are not only threatened with the obligation to "maintain according to their need" the persons thus married during the three years of imprisonment in the workhouse ordained for this "crime"—we learn from this passage that the "crime" of marrying without a license was subject to this penalty—but must also subsequently support both the married couple and their children. Moreover they must deposit 100 Reichsthalers [about $72] with the court "pro aquilali [aequali?] satisfactione et indemnisatio nepublica [re publica?]," after which, of course, the obligation of maintenance ceases; with this fine the authorities shall purchase property for the married couple so far as is practicable and suitable to the circumstances.

Article 13 of the edict admonishes the magistrates "not to allow any persons incapable of supporting themselves to intermarry"; to certain categories, namely, "cottagers, lodgers, day laborers, handy-men and the like," a marriage license shall not be granted without the express consent of the parish on which in case of necessity the support of the married couple would fall. The burdened parish shall have recourse for three years against any officials who permit a marriage contrary to this enactment, in which case the latter shall themselves be subject to the obligation of maintenance.

The edict, however, also provides for the contingency that people, in order to escape the foregoing burdensome regulations, should contract marriages abroad. Those who evade the law in this way shall "like other criminals forfeit the protection of the state for ever, and accordingly be punished in every way like alien vagabonds." (pp. 7-8)

The Bavarian Jews were subjected to still more stringent marriage restrictions than were their Christian fellow-subjects. According to Higby,[2]

[2] Higby, C. P. The religious policy of the Bavarian government during the Napoleonic period. *Studies in History, Economics and Public Law,* Vol. 85, No. 1. *New York* (Columbia University), 1919. Pp. 346.

> The regulations issued by the government in 1805 for the Jews of Munich furnish a good example of the situation of the Bavarian Jews between 1799 and 1813. . . . . Each family received a number which was to be handed down, ordinarily, from father to son. Only one child [son?] in a family, as a usual thing, was to be permitted to marry, and to this child the family number was to be handed over. Two children from a family were permitted to marry only when there had been, by some chance, a decrease in the total number of Jewish families in the city. Widows were permitted to remarry only when they had no children. They were permitted to marry foreigners only when a considerable increase in the wealth of the state could result from the marriage. No marriage of a Jew was to be allowed without the previous investigation by and consent of the police; and every Jew wishing to marry was required to prove that he owned property of the value at least of one thousand Reichsthalers. (pp. 151-152)

Unfortunately, these rigorous restrictions did not seem to have much effect on the plague of beggars from which Bavaria suffered. Unfortunately, also, however successful a government may be in prohibiting marriages, no government has ever found an effective method of preventing extramarital copulation, and these well meant laws were held by common opinion to be one of the chief causes of the high rate of illegitimacy which characterized the Bavarian people.

JOHN R. MINER

RECENT LITERATURE

USEFUL IN THE STUDY OF HUMAN BIOLOGY

In this department will be noticed the books and reprints which are currently received at the Editorial Office. Publications intended for notice here should be addressed to Dr. Raymond Pearl, Department of Biology, School of Hygiene and Public Health, 1901 East Madison Street, Baltimore, Maryland, U. S. A.

I. EVOLUTION AND GENETICS

1. *Evolution—General Principles*

BURKE, JOHN B. The Emergence of Life. Being a Treatise on Mathematical Philosophy and Symbolic Logic by which a New Theory of Space and Time is Evolved. *New York* (Oxford University Press), 1931. Pp. ix + 396. 5½ × 8¾ inches. $7.50.

CLARK, ALEX. The Cool-Water Timms Point Pleistocene Horizon at San Pedro, California. Transactions of the San Diego Society of Natural History, Vol. VII, No. 4. *San Diego, Calif.* (San Diego Society of Natural History), 1931. Pp. 18 + map and chart. 6¾ × 10⅛ inches.

MCATEE, W. L. Effectiveness in Nature of the So-Called Protective Adaptations in the Animal Kingdom, Chiefly as Illustrated by the Food Habits of Nearctic Birds. Smithsonian Miscellaneous Collections, Volume 85, Number 7. *Washington* (Smithsonian Institution), 1932. Pp. 201. 6¼ × 9½ inches. 75 cents (paper). [Bibliography of 57 pages].

2. *Human Evolution*

GREGORY, WILLIAM K. The African Anatomical Expedition of Columbia University and the American Museum of Natural History. *Columbia University Quarterly*, Vol. XXIII, pp. 79-89, 1931.

REYNOLDS, EDWARD. The Evolution of the Human Pelvis in Relation to the Mechanics of the Erect Posture. Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. XI, No. 5. *Cambridge, Mass.* (Peabody Museum, Harvard Univ.), 1931. Pp. 80. 6½ × 9½ inches. $1.00 (paper). [Bibliography of 12 titles].

3. *Prehistory and Archeology*

COLLINS, HENRY B., JR. Excavations at a prehistoric Indian village site in Mississippi. *Proceedings of the United States National Museum*, Vol. 79, pp. 1-22, plates 1-13, 1932.

KEMPTON, J. H. Maize, the plant-breeding achievement of the American Indian. *Old and New Plant Lore* (*Smithsonian Scientific Series*), Vol. 11, pp. 319-348, 1931. [Selected bibliography of 8 titles].

4. *Genetics—General Principles*

GOLDSCHMIDT, RICHARD A. Gibt es eine Vererbung erworbener Eigenschaften? *Zeitschrift Züchtungskunde*, Band 6, pp. 161-170, 1931.

STURTEVANT, A. H., and T. DOBZHANSKY. Contributions to the Genetics of Certain Chromosome Anomalies in Drosophila Melanogaster. Carnegie Institution of Washington Publication No. 421. *Washington* (Carnegie Institution), 1931. Pp. 81. 6⅞ × 10 inches. $1.75 (paper); $2.25 (cloth). [Bibliography of 10 titles].

5. *Human Heredity*

BAUER, JULIUS. Krebs und Vererbung. *Wiener klinische Wochenschrift*, 1931, Nr. 5, pp. (of reprint) 16. [Bibliography of 27 titles].

BAUER, JULIUS, and ALFRED VOGL. Psoriasis und Gelenkleiden. Zugleich ein Beitrag zur Kenntnis des hereditären Hydrops articulorum intermittens. *Klinische Wochenschrift*, Jahrg. 10, pp. 1700-1705, 1931. [Bibliography of 17 titles].

CREW, F. A. E. Mental deficiency. A discussion of the genetic background. *Eugenics Review*, Vol. XXIII, pp. 299-303, 1932.

SNYDER, LAURENCE H. Inherited taste deficiency. *Science*, Vol. 74, pp. 151-152, 1931.

SNYDER, LAURENCE H. Linkage in man. *Eugenical News*, Vol. XVI, pp. 117-119, 1931. [Bibliography of 18 titles].

SNYDER, LAURENCE H. Studies in human inheritance. V. Multiple allelomorphism as opposed to linkage in blood group heredity. *American Naturalist*, Vol. LXV, pp. 332-342, 1931. [Bibliography of 9 titles].

STRANDSKOV, H. H. A statistical study of the relative goodness of fit of the two proposed theories of human blood group inheritance. *Journal of Immunology*, Vol. XXI, pp. 261-277, 1931. [Bibliography of 31 titles].

6. *Eugenics*

DAVENPORT, C. B. Annual report of the Director of the Department of Genetics. *Carnegie Institution of Washington Yearbook* No. 30, pp. 37-73, 1931.

II. GROUP BIOLOGY

1. *Environment of Man*

ANDERSSON, THOR. Statistics of agriculture in Sweden. *Nordic Statistical Journal*, Vol. 2, pp. 269-274, 1930.

CAUSEY, DAVID. Uninvited Guests. A Short Account of the Animals Living On or In Us. *New York* (Alfred A. Knopf), 1932. Pp. ix + 120. 5 × 7¼ inches. $2.00.

GOWANLOCH, JAMES N., and J. B. DAUENHAUER, JR. Sea Fishes and Sea Fishing in Louisiana, Including Recipes for the Preparation of Sea Foods. State of Louisiana, Department of Conservation, Bulletin No. 21. *New Orleans* (Department of Conservation), 1932. Pp. 187. 6 × 9 inches. Free.

NEW YORK COMMISSION ON VENTILATION. Final contribution of the New York Commission on Ventilation. *Milbank Memorial Fund Quarterly Bulletin*, Vol. X, pp. 33-40, 1932.

RICH, WILLIS H., and EDWARD M. BULL. Statistical Review of the Alaska Salmon Fisheries. Part III: Prince William Sound, Copper River, and Bering River. U. S. Department of Commerce, Bureau of Fisheries, Bulletin No. 7. *Washington* (U. S. Government Printing Office), 1932. Pp. 60. 7⅜ × 11 inches. 20 cents (paper).

SANDERSON, E. DWIGHT. Insect Pests of Farm, Garden and Orchard. Third Edition, Revised and Enlarged by Leonard M. Peairs. *New York* (John Wiley and Sons), 1931. Pp. vii + 568. 5¾ × 9 inches. $4.50 net.

SURFACE, FRANK M., and RAYMOND L. BLAND. American Food in the World War and Reconstruction Period. Operations of the Organizations under the Direction of Herbert Hoover 1914 to 1924. *Stanford University* (Stanford University Press), 1931. Pp. xxiii + 1033. 8½ × 11 inches.

TOWNSEND, CHARLES H. Where the Nineteenth Century Whaler Made His Catch. *Bulletin New York Zoological Society*, Vol. XXXIV, No. 6. *New York* (New York Zoological Society), 1931. Pp. 6 + folding map. 7½ × 10¼ inches. 30 cents for single copy of Bulletin; $1.50 a year.

WAKSMAN, SELMAN A. Principles of Soil Microbiology. Second Edition, Thoroughly Revised. *Baltimore* (The Williams and Wilkins Co.), 1932. Pp. xxviii + 894. 6 × 9 inches. $10.00.

2. *Public Health and Hygiene*

APPEL, THEODORE B., Secretary. Commonwealth of Pennsylvania Department of Health, Yearbook 1930. *Harrisburg* (Department of Health), 1931. Pp. 245. 6 × 9 inches.

BAYNE-JONES, STANHOPE. Man and Microbes. *Baltimore* (The Williams and Wilkins Co.), 1932. Pp. x + 128. 5 × 7¼ inches. $1.00.

FOSTER, GEORGE S., and LEON G. KRANZ. Health. A Practical Book on Hygiene. *Boston* (The Christopher Publishing House), 1932. Pp. 218. 5½ × 8½ inches. $3.00.

METROPOLITAN LIFE INSURANCE COMPANY. Functions of an industrial eye clinic. *Industrial Health Series* Number 4. (*Policyholders Service Bureau*, Metropolitan Life Insurance Co., N. Y.). Pp. 56. 5⅜ × 7¾ inches.

PEEBLES, ALLON. A survey of the medical facilities of the State of Vermont. An abstract of a report published by the University of Chicago Press. *Abstract of Publication No. 13. The Committee on the Costs of Medical Care, Washington, D. C.* Pp. 18. 6 × 9 inches. January 1932.

PELC, H. J. Rural hygiene in Czechoslovakia. *Les Travaux de l'Institut d'Hygiène Publique de l'État Tchécoslovaque*, Octobre 1931, No. 4, pp. (of reprint) 33.

PROVINCE OF BRITISH COLUMBIA. Thirty-fifth report of the Provincial Board of Health, including twentieth report of medical inspection of schools, year ended June 30th, 1931. *Victoria, B. C.* (Provincial Board of Health), 1931. Pp. 77. 7¼ × 10½ inches.

REED, LOUIS S. Midwives, chiropodists, and optometrists. Their place in medical care. An abstract of a report of the same title published by the University of Chicago Press. *The Committee on the Costs of Medical Care*, Abstract Publication No. 15, pp. 11, 1932.

STILES, CHARLES W. Hookworm disease in certain parts of the South: a new plan of attack. *Southern Medical Journal*, Vol. XXV, pp. 189-192, 1932.

TODD, T. WINGATE. The registration of life's handicaps. (Annual meeting of the Day Nursery and Free Kindergarten Association of Cleveland January 27, 1931). *Brush Foundation Publications*, No. X, pp. 10.

TODD, T. WINGATE. Safeguarding humanity. (Address to the Lakewood College Club November 5, 1930). *Brush Foundation Publications*, No. IX, pp. 13.

WINSLOW, ANNE. The frontier nursing service. *Miscellaneous Contributions on the Costs of Medical Care*: No. 10. Pp. 16. [A selected factual bibliography of studies on the costs of medical care and related topics, of 6 pp.].

YAO, HSUN-YUAN. The second year of the rural health experiment in Ting Hsien, China. *Milbank Memorial Fund Quarterly Bulletin*, Vol. X, pp. 53-66, 1932.

3. *Migration*

FERENCZI, IMRE. Les migrations et les prévisions démographiques et sociales. *Le Assicurazioni Sociali*, Année VII, pp. (of reprint) 40, 1931.

4. *Population*

BEKLEMISCHEV, W. N. Über die Anwendung einiger Grundbegriffe der Biocönologie auf tierische Komponente der Festlandbiocönosen. *Bulletin of Plant Protection*, Vol. I, pp. 277-358, 1931. [In Russian, with a summary in German; bibliography of 84 titles].

LENZ, FRIEDRICH. Lebensraum und Lebensgemeinschaft. Eine Einführung. *Frankfurt a. M. and Berlin* (Otto Salle), 1931. Pp. 183. 5¼ × 7½ inches. 4.32 marks. [Bibliography of 64 titles].

UNION OF SOUTH AFRICA, Office of Census and Statistics, Pretoria. Fourth census of the population of the Union of South Africa, 4th May, 1926. (Enumeration of Europeans only). Report with summaries and analysis of the detailed tables, Parts I to XI. *Pretoria* (The Government Printer), 1931. Pp. xii + 212. 9½ × 12¼ inches. 10s. 6d. (paper).

UNITED STATES DEPARTMENT OF COMMERCE, BUREAU OF THE CENSUS. Fifteenth Census of the United States: 1930. Population Bulletin Second Series. United States Summary. Composition and Characteristics of the Population. *Washington* (U. S. Government Printing Office), 1931. Pp. 79. 9 × 11½ inches. 20 cents (paper).

5. *Mortality and General Vital Statistics*

DEPORTE, J. V., Director. Fifty-first annual report of the State Department of Health. For the year ending December 31, 1930. Vol. 2. Division of Vital Statistics. *Legislative Document, New York* (1931), No. 28. Pp. cxii + 329. 5⅝ × 8¼ inches.

DOWNES, JEAN. The accuracy of official tuberculosis death rates. *Journal of the American Statistical Association*, Vol. XXVI, pp. 393-406, 1931.

DROLET, GODIAS J., and LOWA M. DORR. Reference statistics. Tuberculosis mortality and morbidity, New York City. From reports by courtesy of the *Bureau of Records and the Bureau of Preventable Diseases, New York City Department of Health*, January 1932. 26 pp. 8¼ × 11 inches.

HOFFMAN, FREDERICK L. San Francisco cancer survey. Seventh preliminary report. (Seventeenth, eighteenth, nineteenth and twentieth quarterly reports). *Newark, N. J.* (Prudential Insurance Co.), no date. Pp. 227. 6 × 9 inches.

PELC, HYNEK. La mortalité causée par la tuberculose en Moravie-Silésie après la guerre. *Les Travaux de l'Institut d'Hygiène Publique de l'État Tchécoslovaque*, July 1931, No. 3, pp. (of reprint) 17. [Bibliography of 12 titles].

PEREZ, MANUEL A. Report of the Bureau of Vital Statistics, Porto Rico Health Department, for the year ended December 31, 1930. *Boletin de la Asociación Médica de Pto. Rico*, Año XXIII, pp. (of reprint) 10, 1931.

RIO DE JANEIRO. Annuario de estatistica demographo-sanitaria 1922. Republica dos Estados Unidos do Brasil. Departamento Nacional de Saude Publica. Inspectoria de Demographia Sanitaria. Pp. iii + 234. 7¼ × 10¾ inches. 1931.

WOODS, HILDA M., and C. O. STALLYBRASS. The part played by meteorological conditions on respiratory mortality in Liverpool. *Journal of Hygiene*, Vol. XXXII, pp. 67-78, 1932. [Bibliography of 8 titles].

6. *Morbidity and Epidemiology*

BEKLEMISCHEV, W., A. BRIUKHANOVA and N. SHIPITZINA. Les prémisses de l'épidémiologie et de la prophylaxie du paludisme à Magnitogorsk. (D'après les observations de 1930). (*Commissariat de la Santé Publique, Département de la Santé Publique de Magnitogorsk*), 1931. Pp. 51. [In Russian with a résumé in French].

BURROUGHS, TRAVIS P. An epidemiological study of a rural outbreak of whooping cough. *Milbank Memorial Fund Quarterly Bulletin*, Vol. X, pp. 41-52, 1932.

HOFFMAN, FREDERICK L. Cancer in the North American Negro. *American Journal of Surgery*, Matas Birthday Volume, October 1931, pp. 229-263.

7. *Natality, Fecundity, Fertility*

DEPORTE, J. V. The prevalent hour of stillbirth. *American Journal of Obstetrics and Gynecology*, Vol. XXIII, pp. 31-37, 1932.

DESHPANDE, S. R., and G. S. GHURYE. A study of some vital problems relating to the working class population of Bombay. *Journal of Indian Economics*, October 1931, pp. 161-170.

SYDENSTRICKER, EDGAR. A study of the fertility of native white women in a rural area of western New York. *Milbank Memorial Fund Quarterly Bulletin*, Vol. X, pp. 17-32, 1932.

WICKSELL, S. D. Nuptiality, fertility, and reproductivity. *Skandinavisk Aktuarietidskrift*, 1931, pp. 125-157.

8. *Birth Control*

DICKINSON, ROBERT L., and LOUISE S. BRYANT. Control of Conception. An Illustrated Medical Manual. *Baltimore* (The Williams and Wilkins Co.), 1931. Pp. xii + 290. 6 × 9 inches. $4.50. [Bibliography of 167 titles].

KNOPF, S. ADOLPHUS. The myth about sterility following contraceptive methods and some new developments in the birth control movement. *Medical Journal and Record*, October 15, 1930, pp. 368-371.

SANGER, MARGARET, and HANNAH M. STONE, Edited by. The Practice of Contraception. An International Symposium and Survey. With a Foreword by Robert L. Dickinson. *Baltimore* (The Williams and Wilkins Co.), 1931. Pp. xviii + 316. 5¾ × 8⅜ inches. $4.00.

STOPES, MARIE C. Preliminary notes on various technical aspects of the control of conception. Based on the analysed data from ten thousand cases attending the pioneer Mothers' Clinic London. *London* (Mothers' Clinic for Constructive Birth Control), 1930. Pp. 46. 5½ × 8⅜ inches.

9. *Marriage and Divorce*

[Vacant]

III. BEHAVIOR

1. *Racial Psychology*

HUXLEY, ALDOUS. Brave New World. *Garden City, N. Y.* (Doubleday, Doran and Co.), 1932. Pp. 311. 5¼ × 7¾ inches. $2.50.

PATON, STEWART. Prohibiting Minds and the Present Social and Economic Crisis. *New York* (Paul B. Hoeber), 1932. Pp. xi + 198. 5 × 7½ inches. [Bibliography of 23 titles].

2. *Individual Psychology*

HENDERSON, LAWRENCE J. An approximate definition of fact. *University of California Publications in Philosophy*, Vol. 14, pp. 179-200, 1932.

TODD, T. WINGATE. Compensations in life. (Annual Address Girls' Bureau of Cleveland, March 26, 1931). *Brush Foundation Publications*, No. XIV, pp. 8.

3. *Psychiatry*

v. WYSS, WALTER H. Körperlich-seelische Zusammenhänge in Gesundheit und Krankheit. Ein Beitrag zur medizinischen Psychologie. (With a Foreword by W. R. Hess). *Leipzig* (Georg Thieme), 1931. Pp. x + 128. 5¾ × 8¼ inches. 6.50 marks (paper).

4. *History and Biography*

AYRES, CLARENCE. Huxley. *New York* (W. W. Norton and Co.), 1932. Pp. vii + 254. 5½ × 8⅛ inches. $3.00. [Bibliography of 23 titles].

BARBOUR, THOMAS. To Leonhard Stejneger. *The Stejneger Anniversary Number of Copeia*, 1931. Pp. (of reprint) 2.

v. BORTKIEWICZ, L. Lexis und Dormoy. *Nordic Statistical Journal*, Vol. 2, pp. 37-54, 1930.

BRITTEN, JAMES, and GEORGE S. BOULGER, Compiled by. A Biographical Index of Deceased British and Irish Botanists. Second Edition, Revised and Completed by A. B. Rendle. *London* (Taylor and Francis), 1931. Pp. xxii + 342. 5½ × 8½ inches. 15 shillings net.

COMPTON, KARL T. An American Faraday. *Technology Review* (Massachusetts Institute of Technology), Vol. XXXIII, pp. 3-7, 1931.

EISLER, ROBERT. The Messiah Jesus and John the Baptist According to Flavius Josephus' Recently Rediscovered 'Capture of Jerusalem' and the Other Jewish and Christian Sources. English Edition by Alexander H. Krappe. *New York* (The Dial Press), 1931. Pp. xxviii + 638. 5½ × 8¾ inches. $10.00.

HARGRAVE, LYNDON L. Oraibi: A brief history of the oldest inhabited town in the United States. *Museum Notes, Museum of Northern Arizona, Flagstaff*, Vol. 4, pp. 1-8, 1932. 10 cents. [Bibliography of 12 titles].

HILL, NORMAN L. International commissions of inquiry and conciliation. *International Conciliation* No. 278. *Worcester, Mass.* (Carnegie Endowment for International Peace), 1932. Pp. 54. 5 × 7¾ inches. 5 cents (paper).

KELLY, HOWARD A. John Whitridge Williams (1866-1931). *American Journal of Surgery*, New Series, Vol. XV, pp. 169-174, 1932.

LILLIE, F. R., and SHIGEO YAMANOUCHI. Shosaburo Watase. *Science*, Vol. LXXI, pp. 577-578, 1930.

MURCHISON, CARL, Edited by. A History of Psychology in Autobiography. Volume II. By Benjamin Bourdon, James Drever, Knight Dunlap, Giulio C. Ferrari, Shepherd I. Franz, Karl Groos, Gerardus Heymans, Harold Höffding, Charles H. Judd, C. Lloyd Morgan, Walter B. Pillsbury, Lewis M. Terman, Margaret F. Washburn, Robert S. Woodworth, Robert M. Yerkes. *Worcester, Mass.* (Clark University Press), 1932. 6 × 9 inches. Pp. xvii + 407. $5.00.

PETERSON, HOUSTON. Huxley. Prophet of Science. *New York* (Longmans, Green and Co.), 1932. Pp. xiii + 338. 5½ × 8½ inches. $3.50. [Bibliography of 6 pages].

ROBINSON, VICTOR. The Story of Medicine. *New York* (Albert and Charles Boni), 1931. Pp. 527. 5¾ × 8¾ inches. $5.00. [22 pages of Bibliographical Notes].

TERRY, ROBERT J. Contributors to Medicine. Address before the *St. Louis Medical Society*, November 24, 1931, on the occasion of the Golden Jubilee Celebration in honor of those members of the Society who have completed fifty or more years of continuous service to the community. Pp. (of reprint) 3.

TERRY, ROBERT J. Dr. John Sappington, pioneer in the use of quinine in the Mississippi Valley. *Proceedings Tercentenary Celebration of Cinchona*, pp. 160-185, 1931.

5. *Sociology, Law, Politics, and Religion*

ASSOCIATION AGAINST THE PROHIBITION AMENDMENT. Handy digest of the Wickersham report. Prepared by the Association Against the Prohibition Amendment (*National Press Bldg., Washington*), Research Department, John C. Gebhart, Director. January 1932. Pp. 17.

COMMONWEALTH FUND (The). Annual report for the year ending September 30, 1931. *41 East Fifty-seventh St., New York* (The Commonwealth Fund), 1932. Pp. 66. 5⅞ × 9 inches.

FOSDICK, RAYMOND B., and JOHN W. DAVIS. Judicial World Organization. A way of escape from war, by Raymond B. Fosdick. The World Court settles the question, by John W. Davis. *International Conciliation*, No. 277. *Worcester, Mass.* (Carnegie Endowment for International Peace), 1932. Pp. 37. 5 × 7¾ inches. 5 cents (paper).

KELLER, ALBERT G. Societal Evolution. A Study of the Evolutionary Basis of the Science of Society. Revised Edition. *New York* (The Macmillan Co.), 1931. Pp. ix + 419. 5 × 7¼ inches. $2.50.

OLIPHANT, HERMAN. Facts, opinions, and value judgments. *Texas Law Review*, Vol. X, pp. 127-139, 1932.

REISER, OLIVER L. The biological origins of religion. *Psychoanalytic Review*, Vol. XIX, pp. (of reprint) 1-22, 1932.

REISER, OLIVER L. Energy the soul of matter. *Journal of Religion*, Vol. XII, pp. 61-79, 1932.

TODD, T. WINGATE. Life's impact and youth's adjustment. (Opening lecture Health Education Foundation, Academy of Medicine of Cleveland, Allen Memorial Library, Cleveland, Ohio, March 22, 1931). *Brush Foundation Publications*, No. XIII, pp. 19.

TROUT, DAVID M. Religious Behavior. An Introduction to the Psychology of Religion. *New York* (The Macmillan Co.), 1931. Pp. xiv + 528. 5¾ × 8½ inches. $4.00. [Bibliography of 310 titles].

WEINMANN, GEORGE H. A Compendium of the Statute Law of Coroners and Medical Examiners in the United States. Issued under the Auspices of the Committee on Medicolegal Problems. Bulletin of the National Research Council No. 83. *Washington* (National Research Council), 1931. Pp. 240. 6¾ × 9¾ inches. $3.00 (paper).

WHITTON, JOHN B. What follows the pact of Paris? *International Conciliation*, No. 276. *Worcester, Mass.* (Carnegie Endowment for International Peace), 1932. Pp. 48. 5 × 7¾ inches. 5 cents (paper).

ZUCKERMAN, S. The Social Life of Monkeys and Apes. *New York* (Harcourt, Brace and Co.), 1932. Pp. xii + 356. 5½ × 8½ inches. $3.75. [Bibliography of 402 titles].

6. *Economics*

AMES, ADELBERT, JR. Progress and prosperity: a suggested program. With a commentary by Philip Cabot. *Supplement to the Dartmouth Alumni Magazine*, Vol. XXIV, pp. 1-15, 1932.

ASSOCIATION AGAINST THE PROHIBITION AMENDMENT. Prohibition and the deficit. Prepared by the Association Against the Prohibition Amendment (*National Press Bldg., Washington*). January 1932. Pp. 10.

ASSOCIATION AGAINST THE PROHIBITION AMENDMENT. Temperance by taxation. How Denmark does it. Prepared by the Association Against the Prohibition Amendment (*National Press Bldg., Washington*). March 1932. Pp. 29.

CAROTHERS, NEIL. Silver—a Senate racket. *Lehigh University Publication*, Vol. VI, No. 1. *The Institute of Research*, Circular No. 70. *Studies in the Humanities*, No. 13. *Bethlehem, Pa.* (Lehigh University), 1932. Pp. 12. 6 × 9 inches. 10 cents (paper).

DUBLIN, MARY. The amount of life insurance in the United States. *Miscellaneous Contributions on the Costs of Medical Care*: Number 11, Pp. 14. 1932. [Selected Factual Bibliography of Studies on the Costs of Medical Care and Related Topics, January 1, 1930 to April 30, 1931, 3 pp.].

LELAND, R. G. Income from medical practice. *Journal of the American Medical Association*, Vol. 96, pp. 1683-1691, 1931.

TOWER, RALPH B. Luxury taxation and its place in a system of public revenues. Special report of the State Tax Commission, No. 4. *Albany* (Department of Taxation and Finance), 1931. Pp. 235. 5⅜ × 9 inches.

WARREN, WHITNEY. "The Vicious Circle." Why is the Treasury worse than empty and confidence absolutely lacking? First Letter: *New York Times*, Nov. 11, 1931; pp. (of reprint) 8. Second Letter: pp. (of reprint) 11, Dec. 27, 1931. Third Letter: pp. (of reprint) 12, Jan. 16, 1932. Fourth Letter: pp. (of reprint) 12, Jan. 29, 1932. Fifth Letter: pp. (of reprint) 10, Feb. 19, 1932. Sixth Letter (specially dedicated to the Legislatures of those states exploited by the Federal Government): pp. (of reprint) 10, March 14, 1932. *Whitney Warren*, 16 East 47th St., New York.

7. *Education*

LIBRARY OF CONGRESS. Report of the Librarian of Congress for the Fiscal Year Ending June 30, 1931. *Washington* (U. S. Government Printing Office), 1931. Pp. vii + 463. 5¾ × 9 inches. $1.00.

8. *Ethnology, Cultural Anthropology, Travel and Exploration*

FORTUNE, R. F. Sorcerers of Dobu. The Social Anthropology of the Dobu Islanders of the Western Pacific. With an Introduction by B. Malinowski. *New York* (E. P. Dutton and Co.), 1932. Pp. xxviii + 318. 5¾ × 8½ inches. $5.00.

HOUGH, WALTER. The buffalo motive in middle Celebes decorative design. *Proceedings of the United States National Museum*, Vol. 79, pp. 1-8 + 9 plates, 1932.

MARKOWSKI, BRUNO. Die materielle Kultur des Kabulsgebietes. *Leipzig* (Verlag Asia Major), 1932. Pp. viii + 154 + 35 plates. 7 × 10 inches. 17 marks (paper); 20 marks (cloth).

RONHAAR, J. H. Woman in Primitive Motherright Societies. *Groningen, The Hague* (J. B. Wolters); *London* (David Nutt), 1931. Pp. iv + 541. 5½ × 8½ inches. 9 guilders (Holland); 15 shillings net (England). [Bibliography of 16 pages].

SMITH, HOMER W. Kamongo. *New York* (The Viking Press), 1932. Pp. 167. 5 × 8⅛ inches. $2.00.

TODD, T. WINGATE. Culture conflict and physical inadequacy as bases for misconduct. *Social Forces*, Vol. IX, pp. 497-499, 1931.

TULANE UNIVERSITY. The Department of Middle American Research of the Tulane University of Louisiana. Its activities and its aims. On the occasion of the 89th meeting of the American Association for the Advancement of Science, *New Orleans*. Pp. 39. 6 × 9 inches. January 1932.

IV. SOMATOLOGY AND CONSTITUTION

1. *Anatomy*

ALTSCHULE, MARK D. The changes in the mesonephric tubules of human embryos ten to twelve weeks old. *Anatomical Record*, Vol. 46, pp. 81-91, 1930. [Bibliography of 13 titles].

ESTRADA, ADÉLAIDE. Les cellules de Rieder dans le sang des syphilitiques. *Comptes rendus des séances de la Société de biologie (Société portugaise de biologie)*, Tome CII, pp. 251-252, 1929.

ESTRADA, ADÉLAIDE. Noyaux libres dans le sang circulant. *Comptes rendus des séances de la Société de biologie (Société portugaise de biologie)*, Tome CI, pp. 397-398, 1929.

ESTRADA, ADÉLAIDE. Recherches sur les corps de Jolly. *Comptes rendus des séances de la Société de biologie (Société portugaise de biologie)*, Tome XCVIII, pp. 1159-1160, 1928.

HIRASAWA, KO. Untersuchungen über das periphere Nervensystem. Plexus brachialis und die Nerven der oberen Extremität. *Arbeiten aus der dritten Abteilung des Anatomischen Institutes der Kaiserlichen Universität Kyoto* (edited by Seigo Funaoka), Serie A, Heft 2. Pp. 190. 7 × 10¼ inches. [Bibliography of 16 titles].

KROGMAN, W. M. Studies in growth changes in the skull and face of anthropoids. I. The eruption of the teeth in anthropoids and Old World apes. *American Journal of Anatomy*, Vol. 46, pp. 303-313, 1930. [Bibliography of 10 titles].

KROGMAN, W. M. Studies in growth changes in the skull and face of anthropoids. II. Ectocranial and endocranial suture closure in anthropoids and Old World apes. *American Journal of Anatomy*, Vol. 46, pp. 315-353, 1930. [Bibliography of 21 titles].

KROGMAN, W. M. Studies in growth changes in the skull and face of anthropoids. III. Growth changes in the skull and face of the gorilla. *American Journal of Anatomy*, Vol. 47, pp. 89-115, 1931. [Bibliography of 53 titles].

KROGMAN, W. M. Studies in growth changes in the skull and face of anthropoids. IV. Growth changes in the skull and face of the chimpanzee. *American Journal of Anatomy*, Vol. 47, pp. 325-342, 1931. [Bibliography of 3 titles].

KROGMAN, W. M. Studies in growth changes in the skull and face of anthropoids. V. Growth changes in the skull and face of the orang-utan. *American Journal of Anatomy*, Vol. 47, pp. 343-365, 1931. [Bibliography of 4 titles].

STREETER, G. L. Annual Report of the Director of the Department of Embryology. *Year Book of the Carnegie Institution of Washington* No. 30, for the year 1930-31, pp. 3-36. (Issued December 10, 1931).

TODD, T. WINGATE. The anatomical features of epiphysial union. *Child Development*, Vol. I, pp. 186-194, 1930. [Bibliography of 17 titles].

TODD, T. WINGATE. Facial growth and mandibular adjustment. *International Journal of Orthodontia, Oral Surgery and Radiography*, Vol. XVI, p. 1243, 1930. [Bibliography of 13 titles].

TODD, T. WINGATE. The roentgenographic appraisement of skeletal differentiation. *Child Development*, Vol. I, pp. 298-310, 1930. [Bibliography of 7 titles].

WILLIS, THEODORE A. The separate neural arch. *Journal of Bone and Joint Surgery*, October 1931, pp. (of reprint) 15. [Bibliography of 13 titles].

2. *Physical Anthropology and Anthropometry*

DANFORTH, C. H. Numerical variation and homologies in vertebrae. *American Journal of Physical Anthropology*, Vol. XIV, pp. 463-481, 1930. [Bibliography of 28 titles].

DANFORTH, C. H. Some racial and hereditary characteristics of hair. *Eugenical News*, Vol. XV, pp. 35-37, 1930.

DAVENPORT, C. B. Individual vs. mass studies in child growth. *Proceedings of the American Philosophical Society*, Vol. LXX, pp. 381-389, 1931.

GRAF, LUCIA ANNY. Über eine Schädelserie aus West-Neu-Guinea. Ein Beitrag zur Rassenkunde von Melanesien. Inaugural-Dissertation. Begutachtet von Herrn Prof. Dr. O. Schlaginhaufen. *Archiv der Julius Klaus-Stiftung für Vererbungsforschung, Sozialanthropologie und Rassenhygiene*, Band VI, pp. 123-276 + 4 plates, 1931.

HERSKOVITS, MELVILLE J. The Anthropometry of the American Negro. *New York* (Columbia University Press), 1930. Pp. xv + 283. 6¼ × 9½ inches. $4.00. [Bibliography of 85 titles].

KÄSSBACHER, MAX. Beitrag zur Morphologie der Neuguinea-Schädel. *Zeitschrift für Anatomie und Entwicklungsgeschichte*, Bd. 96, pp. 453-472, 1931.

MILLER, GERRIT S., JR. Human hair and primate patterning. *Smithsonian Miscellaneous Collections*, Vol. 85, No. 10 (Publication 3130), 1931. Pp. 13 + 5 plates. 6¼ × 9½ inches.

SCHLAGINHAUFEN, OTTO. Die anthropologische Untersuchung an den schweizerischen Stellungspflichtigen. V. Bericht, 1931. *Bulletin der Schweizerischen Gesellschaft für Anthropologie und Ethnologie*, Jahrg. 8, pp. (of reprint) 4. 1931/32.

SCHLAGINHAUFEN, OTTO. Der Schädel des Mathematikers Ludwig Schläfli. *Mitteilungen der Naturforschenden Gesellschaft in Bern* aus dem Jahre 1931. Pp. 35-66 + 4 plates. [Bibliography of 21 titles].

SOCIÉTÉ SUISSE D'ANTHROPOLOGIE ET D'ETHNOLOGIE. Bulletin der Schweizerischen Gesellschaft für Anthropologie und Ethnologie, Jahrg. 8, 1931/32, pp. 1-18. *Zurich 7* (Siège de la Société; Institut anthropologique de l'Université, Plattenstr. 9). 2 francs.

SOMMERFIELD, W. A. Studies in the alimentary canal of man. IX. The calculation of gastric volume. *Journal of Laboratory and Clinical Medicine*, Vol. XVII, p. 240, 1931. [Bibliography of 9 titles].

TODD, T. WINGATE. An anthropologist's study of negro life. (Address delivered before the Association for the Study of Negro Life and History, Cleveland, Ohio, October 27th, 1930). *Brush Foundation Publications*, No. VIII, pp. 8.

TODD, T. WINGATE. Assessing the hazards of childhood. *Journal of Medicine, Cincinnati, Ohio*, October 1931, pp. 1-3.

TODD, T. WINGATE. Differential skeletal maturation in relation to sex, race, variability and disease. *Child Development*, Vol. II, pp. 49-65, 1931. [Bibliography of 20 titles].

TODD, T. WINGATE. The orthodontic value of research and observations in developmental growth of the face. *The Angle Orthodontist*, July 1931. Pp. (of reprint) 4.

TODD, T. WINGATE, and BARBARA TRACY. Racial features in the American Negro cranium. *American Journal of Physical Anthropology*, Vol. XV, pp. 53-110, 1930. [Bibliography of 22 titles].

WOLFF, GEORG. Die Nachwirkung der Kriegshungerperiode auf das Schulkinderwachstum. Eine schulärztlichanthropometrische Wachstumsstudie nach Einschulungsuntersuchungen in Berlin nebst Anleitung zur statistischen Behandlung biologischer Reihenmerkmale. *Leipzig* (Leopold Voss), 1932. Pp. vii + 72. 6⅝ × 9⅝ inches. 8.50 marks.

WRIGHT, SEWALL. Review of "The Measurement of Man," by J. A. Harris, C. M. Jackson, D. G. Paterson and R. E. Scammon. Minneapolis: The University of Minnesota Press. 1930. 215 pp. *Journal of the American Statistical Association*, Vol. XXVI, pp. 358-360, 1931.

3. *Constitution*

ASCHNER, BERTA. Beziehungen der Fettsucht zu arteriellem Hochdruck, Diabetes mellitus und Cholelithiasis. *Zeitschrift für Klinische Medizin*, 116. Band, pp. 669-679, 1931. [Bibliography of 18 titles].

ASCHNER, BERTA. Zur klinischen Bedeutung des "Status Degenerativus." *Klinische Wochenschrift*, Jahrg. 10, pp. 1981-1985, 1931. [Bibliography of 5 titles].

BAUER, JULIUS. Beeinflussung der Tuberkulose durch Konstitution und Krankheiten. Einfluss der Konstitution auf den Ablauf der tuberkulösen Infektion. *Handbuch der Kindertuberkulose*, herausgegeben von Prof. Dr. St. Engel und Prof. Dr. Cl. Pirquet, (Georg Thieme Verlag, Leipzig), pp. 1495-1504. [Bibliography of 48 titles].

BAUER, JULIUS. Entfettungs- und Mastkuren. *Wiener Medizinische Wochenschrift*, Nr. 51, pp. (of reprint) 8, 1931.

BAUER, JULIUS. Die konstitutionellen Grundlagen der Herzschwäche. *Die Medizinische Welt*, Nr. 28, pp. (of reprint) 11, 1930. [Bibliography of 19 titles].

BAUER, JULIUS. Therapeutische Bedeutung von Körpergewichtsänderungen. *Fortschritte der Therapie*, Jahrg. 7, pp. 545-549, 1931.

BAUER, JULIUS. Über Fettsucht. *Die Medizinische Welt*, Nr. 40 u. 44, pp. (of reprint) 16, 1930.

BAUER, JULIUS. Ueber den Missbrauch des Konstitutionsbegriffes (zumal in ärztlichen Gutachten). *Deutsche Medizinische Wochenschrift*, Nr. 21, pp. (of reprint) 4, 1930.

BAUER, JULIUS. Ueberfunktion des gesamten Nebennierensystems ohne anatomischen Befund. *Wiener klinische Wochenschrift*, Nr. 19, pp. (of reprint) 13, 1930. [Bibliography of 28 titles].

FRIEDBERG, CHARLES K. Natur und Wesen des Syndroms: Abnorme Knochenbrüchigkeit, blaue Skleren und Schwerhörigkeit. Beiträge zur klinischen Konstitutionspathologie XX. *Zeitschrift für Konstitutionslehre*, 15. Band, pp. 779-792, 1931. [Bibliography of 59 titles].

PATERSON, DONALD G. Physique and Intellect. *New York* (The Century Co.), 1930. Pp. xxvii + 304. 5¼ × 7¾ inches. $2.50.

SILVER, SOLOMON, and JULIUS BAUER. Obesity, constitutional or endocrine? *American Journal of the Medical Sciences*, vol. clxxxi, pp. 769-776, 1931.

TODD, T. WINGATE. The basis of personality. (Staff Meeting of the Hathaway-Brown School, Cleveland, February 6, 1931). *Brush Foundation Publications*, No. XI, pp. 8.

V. PHYSIOLOGY AND BIOCHEMISTRY

1. *Physiology*

BACKMAN, GASTON. Das Wachstumsproblem. *Ergebnisse der Physiologie*, Band 33, pp. 883-973, 1931. [Bibliography of 2½ pages].

BAUER, JULIUS. Über Herzhormone. *Über Kreislauferkrankungen und ihre Behandlung. VIII. Fortbildungs-Lehrgang in Bad Nauheim* (17-19 September 1931), pp. 105-115. [Bibliography of 18 titles].

CANNON, WALTER B. The Wisdom of the Body. *New York* (W. W. Norton and Co.), 1932. Pp. 312. 5½ × 8½ inches $3.50. [List of 37 publications from the Physiological Laboratory in Harvard University, on which the present account is based].

CLOW, ALICE E. SANDERSON. Treatment of dysmenorrhoea by exercise. *British Medical Journal*, January 2, 1932, No. 3704, pp. 4-5. [Bibliography of one title].

CUNY, L., and D. QUIVY. Données Actuelles sur l'Hormone Testiculaire. (Modes d'obtention et de caractérisation). *Paris* (Masson et Cie), 1932. Pp. 76. 5½ × 9 inches. 16 francs (paper). [Bibliography of 280 titles].

GOLDSCHMIDT, RICHARD. Intersexualität und menschliches Zwittertum. *Deutsche medizinische Wochenschrift*, Nr. 30, pp. (of reprint) 1-12, 1931.

HANCE, ROBERT T. The Machines We Are. The Principles of Living Phenomena. *New York* (Thomas Y. Crowell Co.), 1932. Pp. xvi + 382. 5¾ × 8⅛ inches. $3.00. ["Suggested Readings," 16 titles].

KUENZEL, WILHELMINE, and T. WINGATE TODD. Studies in the alimentary tract of man. VII. The gastric responses to water and lactic acid. *Journal of Laboratory and Clinical Medicine*, Vol. XVI, p. 141, 1930. [Bibliography of 8 titles].

RANDOIN, LUCIE, and HENRY SIMMONET. Les Vitamines. *Paris* (Armand Colin), 1932. Pp. 220. 4½ × 6¾ inches. 10.50 francs (paper); 12 francs (cloth). [Bibliography of 13 titles].

RIDDLE, OSCAR. New data on the relation of metabolism to sex. *Proceedings of the Second International Congress for Sex Research*, 1930, pp. 180-188.

SCHMIDT, FERDINAND A., and WOLFGANG KOHLRAUSCH. Physiology of Exercise. Authorized Translation from the German by Carl B. Sputh, Assisted by Emil Rath and Leopold Zwarg. *Philadelphia* (F. A. Davis Co.), 1931. Pp. 216. 5¾ × 8⅝ inches. $2.50.

SOMMERFIELD, W. A., W. M. KUENZEL and T. WINGATE TODD. Studies in the alimentary canal of man. VIII. The time relationship of gastric peristalsis. *Journal of Laboratory and Clinical Medicine*, Vol. XVII, p. 151, 1931. [Bibliography of 3 titles].

TODD, T. WINGATE. The natural history of human growth. Cleveland Museum of Natural History, March 15, 1931. *Brush Foundation Publications*, No. XII, pp. 15.

TODD, T. WINGATE. The practical control of gastric motility. *1930 Minneapolis Proceedings of the Inter-State Postgraduate Medical Assembly of North America*, pp. 549-554, 1931.

2. *Senescence, Senility and Longevity*

GRAY, W. FORBES. Five Score. A Group of Famous Centenarians. *London* (John Murray), 1931. Pp. ix + 322. 5½ × 8½ inches. 12 shillings net.

3. *Biochemistry*

ARMSTRONG, E. F., and K. F. The Glycosides. *New York* (Longmans, Green and Co.), 1931. Pp. vii + 123. 6 × 9½ inches. $4.25. [Bibliography of 9 pages].

HAUROWITZ, FELIX. Fortschritte der Biochemie. II. Teil (1924-1931). *Dresden and Leipzig* (Theodor Steinkopff), 1932. Pp. x + 152. 6 × 8¾ inches. 11 marks (paper); 12 marks (cloth).

HEDGES, ERNEST S. Colloids. *New York* (Longmans, Green and Co.), 1931. Pp. vii + 272. 5½ × 8½ inches. $4.50.

NEEDHAM, JOSEPH. Chemical Embryology. In three volumes. *New York* (The Macmillan Co.), 1931. Pp. xxi + xvi + xiv + 2021. 6¼ × 9⅛ inches. $35.00. [Bibliography of 244 pages].

NORD, F. F., and R. WEIDENHAGEN, Edited by. Ergebnisse der Enzymforschung. I. Band. By A. Bertho, A. Fodor, E. I. Fulmer, W. Grassmann, A. Harden, F. F. Nord, J. H. Northrop, J. H. Quastel, H. Raistrick, H. S. Raper, A. Reid, R. Robinson, K. Suessenguth, James B. Sumner, R. Weidenhagen, R. Wurmser. *Leipzig* (Akademische Verlagsgesellschaft), 1932. Pp. xi + 377. 6 × 9¼ inches. 27 marks (paper); 29 marks (cloth).

PATCH (THE E. L.) COMPANY. Papers Published by the Research Laboratory of The E. L. Patch Company (Boston, Mass.), October 1931. 5 pp. [Bibliography of 46 titles].

4. *Pharmacology*

ROSE, CHARLES L. Studies in the pharmacology of local anesthetics: V. A discussion of the present methods of evaluating local anesthetics. *Anesthesia and Analgesia*, July-August, 1931, pp. (of reprint) 7. [Bibliography of 7 titles].

VI. PATHOLOGY, CLINICAL MEDICINE AND SURGERY

BAUER, JULIUS. Allgemeine Pathogenese innersekretorischen Störungen. *Wiener Medizinische Wochenschrift*, Nr. 49, pp. (of reprint) 10, 1929.

BAUER, JULIUS. Indikationen zum chirurgischen Eingriff bei M. Basedow. *Klinische Wochenschrift*, Jahrg. 10, pp. 626-630, 1931. [Bibliography of 14 titles].

CHEVALLIER, PAUL, and JEAN BERNARD. La Maladie de Hodgkin. Lymphadénie Aleucémique. Lymphosarcome. Pseudo-Leucémie de Bonfils. Morbus Hodgkini de Wilks. Adénie de Trousseau. Granulome Malin de Benda. Lymphogranulome Malin. Lymphogranulomatose Maligne, etc. *Paris* (Masson et Cie), 1932. Pp. 293. 6¾ × 10¼ inches. 75 francs (paper). [Bibliography of 10 pages].

FUNAOKA, SEIGO (Edited by). Experimentelle Tuberkuloseforschung. *Arbeiten aus der dritten Abteilung des Anatomischen Institutes der Kaiserlichen Universität Kyoto*, Serie C, Heft 2, 1931. Pp. 108. 7 × 10¼ inches.

GOIFFON, R. Manuel de Coprologie Clinique. Deuxième Édition. Preface by J.-Ch. Roux. *Paris* (Masson et Cie), 1925. Pp. xi + 260. 5 × 7⅝ inches. 20 francs (paper).

IMPERIAL CANCER RESEARCH FUND. Twenty-ninth annual report, 1930-1931. *Examination Hall, 8-11 Queen Sq., Bloomsbury, London* (Imperial Cancer Research Fund), 1931. Pp. 12. 8¼ × 13¼ inches.

JOHNS HOPKINS HOSPITAL. Forty-second report of the director. Treasurer's report for the fiscal year ending June 30, 1931. Pp. 146. 6 × 9 inches.

MAHAIM, IVAN. Les Maladies Organiques du Faisceau de His-Tawara. Les Syndromes Coronaires. L'Endocardite Septale. L'Infarctus Septal. (Étude Clinique et Anatomique). Preface by L. Michaud. *Paris* (Masson et Cie), 1931. Pp. vi + 595. 6½ × 10 inches. 80 francs (paper). [Bibliography of 1032 titles].

MILLOT, JACQUES. Cicatrisation et Régénération. *Paris* (Armand Colin), 1931. Pp. 204. 4⅜ × 6¾ inches. 10.50 francs (paper); 12 francs (cloth).

REICH, RUDOLPH S. Traumatic dislocation of the shoulder. *Journal of Bone and Joint Surgery*, Vol. xiv, pp. 73-84, 1932. [Bibliography of 8 titles].

RILEY, WILLIAM A., and OSKAR A. JOHANNSEN. Medical Entomology. A Survey of Insects and Allied Forms which Affect the Health of Man and Animals. *New York* (McGraw-Hill Book Co.), 1932. Pp. xi + 476. 5¾ × 9 inches. $4.50. [Bibliography of 31 pages].

STAHL, FRANK A. Origin of Cancer. The Specific Cancer Cell of Carcinoma, Contrasted with the Normal Matrix Embryonal Cellule of Primal Ovum Days, to 8th Week; from which the Cancer Cell is Directly Descended. Cancer not Epithelial. Sarcoma not Connective Tissue. *Hamilton Club, Chicago* (Frank A. Stahl), 1932. Pp. 96. 6 × 9⅛ inches. (paper).

VII. METHODOLOGY

1. *Mensurational and Observational*

YERKES, ROBERT M. Yale Laboratories of Comparative Psychobiology. Comparative Psychology Monographs, Vol. 8, No. 3, Serial No. 38. *Baltimore* (The Johns Hopkins Press), 1932. Pp. 33. 8¾ × 10 inches. 75 cents (paper).

2. *Mathematical and Statistical*

DUNLAP, JACK W., and ALBERT K. KURTZ. Handbook of Statistical Nomographs, Tables, and Formulas. *Yonkers-on-Hudson, N. Y.* (World Book Co.), 1932. Pp. vii + 163. 7½ × 11 inches. $6.00.

HAYS, F. A. The significance of differences in means in repetition experiments. *Poultry Science*, Vol. XI, pp. 14-17, 1932. [Bibliography of 3 titles].

NICEFORO, ALFREDO. Il metodo statistico. Teoria e Applicazioni alle Scienze naturali, alle Scienze sociali, all'Arte. Nuova Edizione, Ampliata. *Messina* (Casa Editrice Giuseppe Principato), 1931. Pp. 6¾ × 9¾ inches. 75 lire (paper).

SCHULTZ, HENRY. Review of "Mathematical Introduction to Economics," by Griffith C. Evans. New York: McGraw-Hill Book Company. 1930. xi, 177 pp. *Journal of the American Statistical Association*, Vol. XXVI, pp. 484-491, 1931.

VIGNERON, H. Manuel des Calculs de Laboratoire. Précision. Discussion et Interprétation des Résultats Expérimentaux à l'Usage des Chimistes, Biologistes et Physico-Chimistes. With a preface by P. Pascal. *Paris* (Masson et Cie), 1931. Pp. 183. 6⅛ × 9⅛ inches. 40 francs (paper).

WINSOR, CHARLES P. The Gompertz curve as a growth curve. *Proceedings of the National Academy of Sciences*, Vol. 18, pp. 1-8, 1932. [Bibliography of 17 titles].

WINSOR, CHARLES P. A comparison of certain symmetrical growth curves. *Journal of the Washington Academy of Sciences*, Vol. 22, pp. 73-84, 1932.

3. *Other and General*

[Vacant]

VIII. GENERAL AND MISCELLANEOUS

CASTALDI, LUIGI. Scritti Biologici. Raccolti da Luigi Castaldi, Volume VI. *Siena* (Stab. Tipografico S. Bernardino), 1931. Pp. 382. 6¾ × 9¾ inches.

MANLEY, R. M. Kinetic Space and its Speculative Consequences. *Cleveland* (R. M. Manley), 1930. Pp. 93. 5 × 7½ inches.

MANLEY, R. M. Synthetic Philosophy. The nature of space, time, matter, mind and life, as it has been portrayed by the world's greatest thinkers, systematized into a rational pattern. *Cleveland* (R. M. Manley), 1931. Pp. iv + 66. 6 × 9 inches.

WHEELER, WILLIAM MORTON. Hopes in the biological sciences. *Proceedings of the American Philosophical Society*, Vol. LXX, pp. 231-239, 1931.

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